PALESTINE EXPLORATION QUARTERLY

Embodying the *Quarterly Statement* of the Palestine Exploration Fund and the *Bulletin* of the British School of Archaeology in Jerusalem

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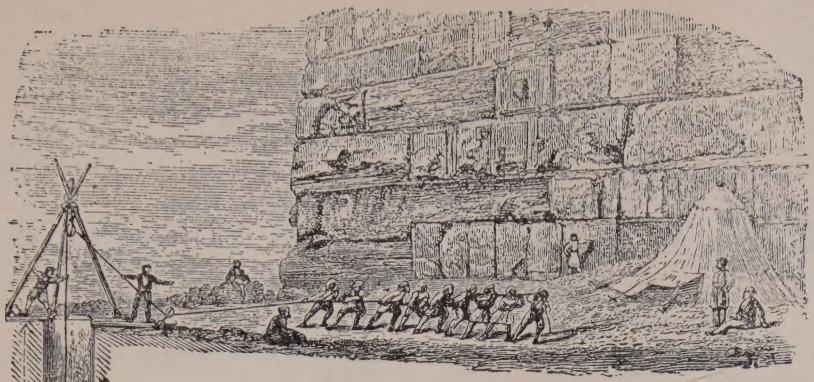

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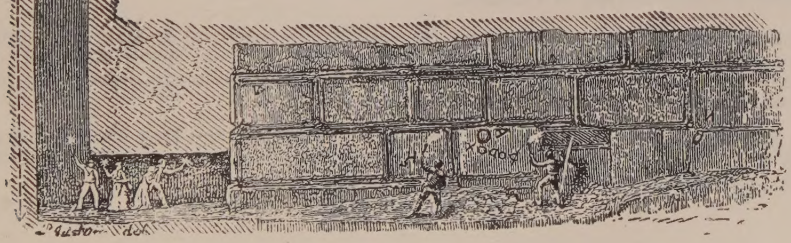

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THE PALESTINE EXPLORATION FUND

NOTES AND NEWS

Dr. Isserlin's interest in the study of Semitic place-names is evidenced by his article in our present issue[1]. Another contribution of his to this subject appeared in the *Proceedings of the Leeds Philosophical Society* (*Literary and Historical Section*), Vol. VIII, Part II (December 1956), pp. 83–110, under the title " Place Name Provinces in the Semitic-Speaking Ancient Near East ". This article shows clearly the links which bind together the West Semitic region as a whole (in spite of subsidiary variations) and mark it off clearly from the East Semitic region. While verbal compound place-names (common in the west) are very rare in East Semitic, the nominal compound class is highly developed.

The latest number of *Syria* to reach us—the first and second fascicles of Vol. XXXIV (1957)—contains two articles of special Palestinian interest: one by Jean Perrot on excavations at Abu Matar, near Beersheba, which have brought to light traces of a chalcolithic civilization (carried there, he suggests, by migrants from Transjordan); the other by Henri Michaud on a fresh study of the seventeen Lachish ostraca which are preserved in London, at the British Museum and the Institute of Archaeology. While the published photographs are, in general, more legible than the ostraca themselves, an examination of the ostraca enables one to check disputed interpretations of the photographed text and places where a scratch or the like might be mistaken for an ink-stroke. On the basis of such an examination, M. Michaud provides a transliteration and translation of a number of the ostraca. In No. 8 he finds a reference to Moab (*m'b*)—*m*' at the end of line 3 and *b* at the beginning of line 4, with a word-divider marking off the word from what precedes and follows. " The importance of this reference ", he says, " will be overlooked by no one "; it is all the more regrettable that the letters preceding and following *m'b* cannot be deciphered with anything like certainty.

[1] Cf. his article on " Hurrian and Old Anatolian Place Names in the Semitic World " in *P.E.Q.*, 1956, pp. 141 ff.

Supplement IV to *Vetus Testamentum* brings together papers read at the International Congress for Old Testament Study at Strasbourg from August 27th to September 1st last year, under the presidency of Professor G. R. Driver. It is full of material of the highest interest to students of Palestinian history and geography. Two papers—by Fr. J. T. Milik and Mgr. P. W. Skehan—are devoted to the Qumran texts. Professor Albright discusses " The High Place in Ancient Palestine " and concludes that " the etymological picture is now sufficiently clear to enable us to set up a basic meaning ' projecting mass of rock, mountain ridge, stone burial cairn ' for Heb. *bâmāh*, ' high place '." Among Old Testament *cruces interpretum* which are illuminated by this new interpretation are Isaiah vi, 13 and liii, 9 (where 1 Q Is. a, reading *bômātô*, " his funerary installation ", in place of MT *be-môtāw*, " in his deaths ", points the way to the true sense). President Mazar of the Hebrew University deals with Shishak's Palestinian campaign; Fr. Vogt with the evidence of the newly-published Babylonian chronicle regarding the battle of Carchemish, the capture of Jerusalem and associated events; Professor Eissfeldt with " Shiloh and Jerusalem " and Professor Widengren on relations between Jews and Iranians during the period of Parthian power. Professor Driver's presidential address contains abundant food for thought; the present writer finds it reassuring to be told that " only a classical education trains the reasoning faculty and brings intuitive and emotional methods under logical control "; his reasoning faculty has been trained by classical study to this point at least, that he recognizes that even of those who have enjoyed the advantage of such a training not all exhibit the same degree of logical power as Professor Driver!

We have received a few issues of a new and unpretentious folder—the *Jerusalem Pontifical Library News-Flash*. The first issue described itself modestly—but optimistically withal—as " O.S. Vol. I, No. 1 (January, 1957) ". It is good to learn in this way of the library's progress.

In the *Jewish Quarterly Review* for January, 1957, Professor Zeitlin maintains his argument for the medieval dating of the Qumran texts—this time with reference to the lately published columns of the Genesis *apocryphon*, formerly and provisionally known as the Lamech apocalypse (see p. 149). This document, he holds, is a medieval midrash, whose author made use of other identifiable midrashic literature and of the Targumim. One expression which, in his view, may reveal more nearly the date at which the text was composed is the statement that Chedorlaomer and his allies, after their victory, made their way *li-medinath darmeseq* (col. 22, l. 5). If this phrase is to be translated " to the city of Damascus ", then, he says, the work is to be dated in the Arabic period, for it is in Arabic that *medina* means " city " (and he quotes midrashic evidence for the application of *medina* to Damascus in the sense of " city "). But is there any reason why *medinath darmeseq* in

our Aramaic text should not have its natural sense of " the ' province ' of Damascus "? As a matter of fact the context here implies that the region around Damascus and not the city itself is intended.

At the end of his article, Professor Zeitlin asks, " Why has the Carbon 14 test not been applied to each of the scrolls? " The answer is, surely, that the application of the test to the scrolls themselves would involve the destruction of irreplaceable texts.

Dr. T. H. Gaster's translation of the Qumran texts, which was noticed in our last issue (p. 87), has now appeared in a British edition, published by Secker and Warburg under the title *The Scriptures of the Dead Sea Sect* (cloth bound, 30*s*.; paper bound, 10*s*. 6*d*.). An appendix to this edition presents translations of texts which have been published since the first edition came out in America—five columns of the Genesis *apocryphon* (see p. 149 below), and the four fragments from Cave 4 published by Mr. J. M. Allegro in the *J.B.L.* of September, 1956. Dr. Gaster points out that the three " messianic " proof-texts of 4Q Testimonia (Deut. xviii, 18 f.; Num. xxiv, 15 ff.; Deut. xxxiii, 8 ff.) " are used by the Samaritans as the stock *testimonia* to the coming of the Taheb, or future ' Restorer '."

At the Annual Meeting of the Palestine Exploration Fund, held on July 11, 1957, Mr. G. Lankester Harding gave a most informative survey of " Recent Discoveries in Jordan," which we hope to print in a later issue of the *Quarterly*. In the course of this survey he gave some account of manuscripts discovered in Cave XI at Qumran. Among the " truly magnificant pieces of scrolls " found in this cave, he said, were a beautiful small scroll of part of Leviticus in proto-Hebrew script, many large pieces of the Apocalypse of the New Jerusalem, an Aramaic Targum of Job, and a very fine, tightly-rolled, nearly complete copy of the Psalter. He mentioned rumours of other good scrolls from the same cave. (From other quarters we hear reports of two nearly complete copies of the Book of Daniel among these.) The manuscripts are said to be the best preserved that have been found at Qumran since the first lot discovered in Cave I, although those in Cave XI were not placed in jars. We look forward avidly to further information about them.

In recent months considerable interest has been aroused by the claim of Professor G. R. Driver and Dr. Cecil Roth to have " unriddled the riddle " of the scrolls. Briefly, their solution is based on the identification of the Teacher of Righteousness with Menahem, grandson of the insurgent Judas of Galilee, and of the Wicked Priest with Eleazer, who as captain of the temple played a leading part in the early stages of the Jewish revolt of A.D. 66. The identification of the Teacher of Righteousness with Menahem was, of course, made a few years ago by H. E. del Medico; but this latest reconstruction differs in a number of points from his. Professor Driver and Dr. Roth have

embodied their findings in a book which will be published before long, and it will receive careful attention in our pages when it appears.

We are constantly receiving fresh evidence, on both sides of the Atlantic, of the re-awakened interest in the Shapira manuscripts which the public discussion of the Qumran texts has stimulated. In this country a defence of " The Genuineness of the Shapira Manuscripts " appeared in *The Times Literary Supplement* of March 22nd, 1957, from the pen of Dr. J. L. Teicher. According to him, their contents represent a Jewish-Christian redrafting of Deuteronomy, dating perhaps from the second or third century. The subject was further discussed by Professor Menahem Mansoor in the Home Service of the B.B.C. on May 26th under the caption, " The First Dead Sea Scroll? " One piece of evidence, however, which carries considerable weight on the other side, is the fact that Shapira's daughter, Myriam Harry, in her slightly fictionalized account of the matter in *La petite fille de Jérusalem* (1914), seems to be quite sure that Clermont-Ganneau (Merle-Vanneau in her transparent change of nomenclature) was right in charging Shapira with forgery. Certainly it would be interesting if the actual manuscripts could be run to earth (they have strangely vanished); a fresh expert examination of them would no doubt impose a speedy settlement on this re-opening of the question.

We are glad to learn that the University of Cambridge has resolved to make a reappointment to the Sir Herbert Thompson Chair of Egyptology, in succession to the late Professor S. R. K. Glanville, and that the Rev. J. M. Plumley, M.A., of King's College, is the new Professor. To Mr. Plumley, who contributed the paper on " Early Christianity in Egypt " to our last issue, we extend our warmest congratulations and good wishes[1].

[1] By an oversight, Professor Plumley received no proof of his article before it was printed, and as a result a number of slips appear in the text which could have been avoided if he had received one. For this we tender him our sincere apologies.

A. The front of the new School building.

B. The garden.

A. The hall, with dining table at end

B. The common room

A. The Library

B. Sorting books in the courtyard

THE BRITISH SCHOOL OF ARCHAEOLOGY IN JERUSALEM
THE NEW SCHOOL BUILDING

By KATHLEEN M. KENYON, *Director*

The British School of Archaeology in Jerusalem is no new body. It is in fact the doyen of the British Schools outside Europe. The Schools of Athens and Rome are evidence of the seniority of the classical lands in archaeological studies, and their foundation marks the new scientific approach to such studies in the late nineteenth and early twentieth centuries.

Archaeological interest in the lands of Western Asia was essentially a nineteenth century growth. In this interest, Palestine always ranked high; though the richness of finds could never equal those of Egypt and Mesopotamia, its position as the land of the Bible gave it an especial appeal. Thus, just as the Palestine Exploration Fund, founded in 1865, was the first society formed to bring together those interested in the exploration of foreign lands, so the School in Jerusalem was the first British School of Archaeology established to train British students in the lands of the East.

The impetus for the foundation of the School was given by the setting up of the British Mandate in Palestine in 1919. It was recognized that the Mandatory Power had highly important cultural responsibilities. The initiative in the foundation of the School was taken jointly by the Palestine Exploration Fund and the British Academy, and the School was thus, like all other Schools, of non-governmental origin, but the obligations of the Mandatory Power to provide for an adequately trained personnel of an Antiquities Department was recognized by a government grant to the School, the first such grant to be given. Professor John Garstang, the first Director of the School, also became the first Director of the Antiquities Department of the Mandatory Government.

Under Professor Garstang, and his successor Mr. J. W. Crowfoot, the School admirably carried out its function of providing training in Palestinian archaeology. Almost all the British officials of the Department of Antiquities in fact received their training as students of the School. But the period between the two wars was a difficult period financially in which to launch such a body. The era in which generous private financial support could be expected was almost ended, with the fortunate exception in the case of the School in Jerusalem of its Treasurer, Mr. (later Sir) Robert Mond. The era in which it is recognized that the value of such institutions as an advertisement of British scholarship overseas is such that they should receive substantial Treasury grants, had not arrived. The initial grant given to the School was

thus suspended in 1926, on the grounds that the Department of Antiquities was now so firmly established that the help of the School was no longer required.

From 1926 onwards, therefore, the School was dependent on private support. Though it continued, until the political troubles in Palestine before the second World War, to make important contributions to research, notably in its share in the excavations of Ophel, Jerash, Samaria and on Mount Carmel, and in the survey initiated in 1936, from 1926 onwards it could no longer afford a School building.

This period in which it was a School in name and spirit, but without any physical abode, only came to an end in 1957. At the end of the war, the British Treasury accepted the view that in modern conditions Schools of Archaeology *in partibus* are worthy of government grants and could not, in modern conditions, continue without them. But at that time conditions in Palestine were too disturbed for archaeological work to be resumed there and the School in Jerusalem did not share in the grants made to the other Schools. When, in 1951, archaeology became once more possible, a grant was made to cover administrative expenses and studentships, and the School resumed work with the excavations at Jericho. But it was not until 1956 that a grant was made on a scale to enable a building to be acquired in Jerusalem. The School owes this grant to the persuasive power of its Chairman, Sir Mortimer Wheeler.

The triumphant moment of the opening of the new School building arrived in March 1957. In July 1956, the lease was taken of the ground floor of a substantial house situated a little east of the Nablus road, near St. George's Collegiate Church (map, fig. 1). It was not till March 1957 that it was possible to move in, but by the end of the month the School may be said to have come formally into an existence with a luncheon party at which we were honoured by the presence of H.E. The British Ambassador and Mrs. Johnston and the Bishop in Jerusalem and Mrs. Stewart.

The School is attractively set in a garden, with a fine avenue of trees stretching down to the front gate (pl. XVIII A, B). It is built round an interior courtyard, along one side of which runs a long hall, which serves as a dining-room (pl. XIX A). From the centre of the hall opens the library, a large, pleasant room, with a bow window looking down the garden (pl. XX A). The common room (pl. XIX B) flanks the courtyard on one side, and in addition to a small office, kitchen, bathroom and servants' room, there is a room for the resident secretary-librarian, and three other large bedrooms. Ordinarily these will serve as bed-sitting rooms for three resident students, but in times of pressure they could accommodate two or three persons. The accommodation is perhaps modest in scale but it is hoped it will be adequate for the time being.

The library of the School has been in existence since the first foundation of the School, having since 1926 been most generously housed by the American

School of Oriental Research. Since 1952, much has been done to bring it up to date. It now contains most of the essential archaeological and historical works on Palestine and surrounding countries, but so far it has not been the policy to attempt to duplicate the excellent theological and linguistic sections

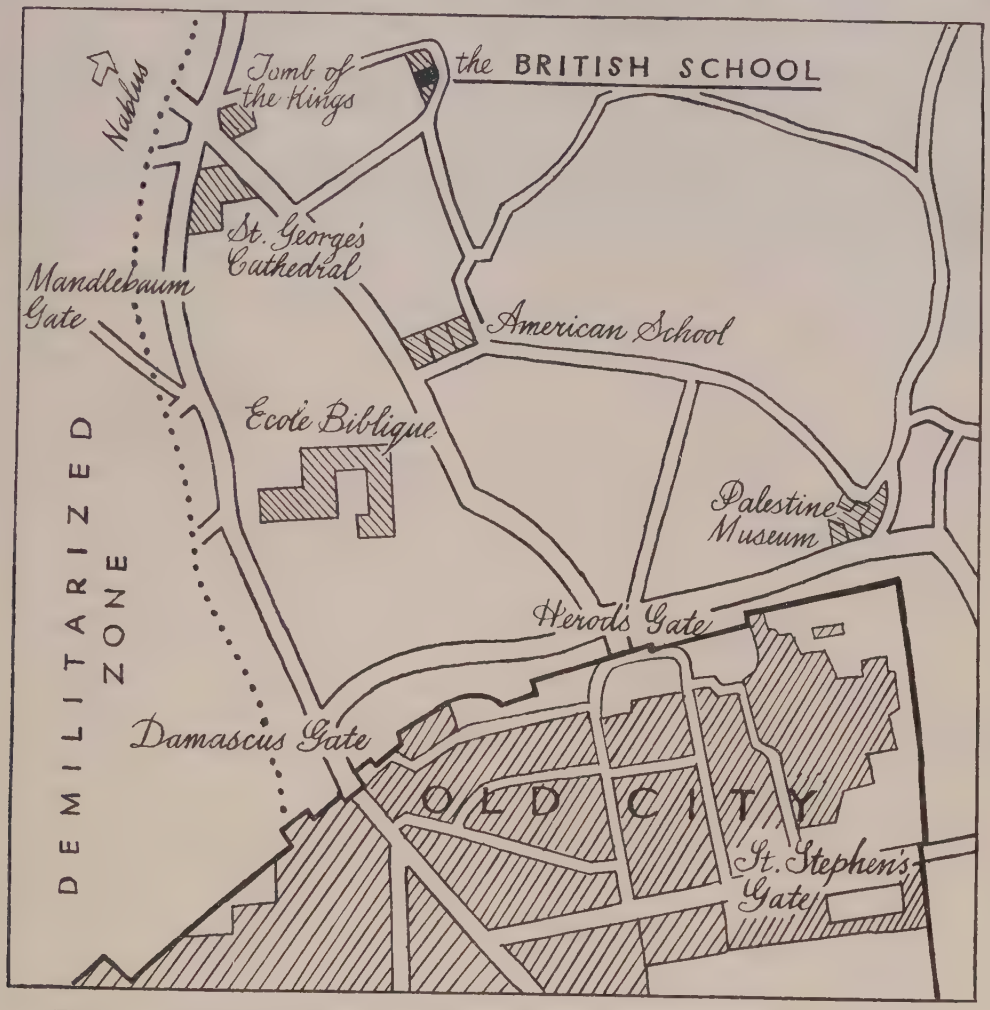


Fig. 1. Map to show position of British School of Archaeology in Jerusalem.

of the American School and other libraries in Jerusalem which are available to students.

Mr. Peter J. Parr has been appointed the first resident Secretary-Librarian of the School. He is in charge of the day-to-day administration, and will assist students in their archaeological work. In the allocation of the accommodation of the School, first preference will of course be given to holders of the

scholarships awarded by the School, and to other students working under the auspices of the School. If accommodation is available, staff and students of other British Schools and of Universities of the British Commonwealth will also be welcome. It is, moreover, hoped that contributors to the British School of Archaeology in Jerusalem and the Palestine Exploration Fund who are visiting Jordan will be able to stay at the School. They will certainly be welcomed as residents if there is room, and in any case the Secretary-Librarian will be glad to give them any help he can in connection with their visit.

We can therefore hope that the new British School building, as a home for our resident students, as the headquarters of British archaeological Expeditions and as a centre for travellers interested in history and archaeology, will make an important contribution to British work in Palestine.

EXCAVATIONS AT JERICHO 1957

By Kathleen M. Kenyon

The 1957 season of excavations at Jericho was on a small scale, as it was not possible to begin work until March. The excavations lasted for four weeks. The staff consisted of Lady Wheeler, draftsman and site supervisor, Mr. P. Dorrell, photographer and site supervisor, Mr. L. Toombs, site supervisor, Mr. P. Parr, site supervisor, Miss G. Talbot, site supervisor (the last two for the end of the period only), Dr. G. Kurth, anthropologist, Miss D. Marshall, registrar, and Miss V. Catleugh, camp supervisor. The expedition was as usual received with courtesy and friendliness by the Department of Antiquities, and our best thanks are due to Dr. Abdul Karim Gharaybeh, Director of Antiquities, for permission to continue the excavations and for all his help.

Work was concentrated primarily on the areas where the greatest progress had already been made in the pre-pottery Neolithic levels. The exception was that a new area was opened to the east of the Neolithic tower, with the hope that in the subsequent season it will be possible to get down to the outer end of the passage in the heart of the tower. Unfortunately, work had to begin with the shifting of a large dump some fifteen feet high. By the end of the season, most of the dump had been removed, and pre-pottery Neolithic levels were reached over approximately half the area.

Both to the north of the tower, in Site F, and to the south, in Site D (see plan, *P.E.Q.* 1956, p. 68, fig. 1) bed-rock had been reached in soundings in 1956, and excavation, in areas considerably restricted by the bulk of the tower and the ever-increasing space required by stairways, had already penetrated well down into the earlier of the two pre-pottery Neolithic phases so far identified, the phase characterized by the use of the hog-backed bricks (see *P.E.Q.* 1956, p. 72). Work was continued in both sites, in the hope that they would be completely cleared in the four weeks available. In neither was this aim achieved, owing to the unexpected complexity of the remains revealed. All these represented buildings in use during the life-time of the tower, during which the deposits against the tower gradually rose to the extent of *c.* 6.50 m.

Site D is the smaller area, for it does not extend so far to the west, and does not reach the line of the successive Neolithic town walls. The area remaining for excavation is only 5.75m. × 3.75m. at the lowest level reached. But even within this area, a surprisingly complex series of structures was identified.

The sounding in Site D which had established the level of bed-rock was immediately adjacent to the tower. It had revealed a series of surfaces

sloping down away from the tower, which appeared to be simple tip lines. But when the area of clearance was enlarged, it emerged that the surfaces had considerably more significance than that of tip lines. The majority of them proved to be associated with structures south of the tower, and since experience elsewhere showed that successive houses were not necessarily in the same position, the remaining surfaces were probably associated with other structural phases lying outside the excavated area.

The buildings were of the same type as those identified last season as being typical of the hog-backed brick phase (*P.E.Q.* 1956, p. 72), and they were all apparently domestic in use. The interior of the house was sunk to a greater or lesser degree beneath the exterior surface. The walls were curved in plan, and tended to have an inward tilt in elevation. In some cases, the houses certainly had at least two rooms, but none of the houses lay completely within the excavated area. One of the best preserved was that at which clearance stopped in 1956 (*P.E.Q.* 1956, pl. X.2). Further examination this year showed that in addition to the timber treads to the steps leading down into the house, sockets of substantial wooden door-posts were preserved in the ends of the walls flanking the steps.

Altogether, twenty-two structural phases of the hog-backed brick period have been identified in Site D. During the period covered by these successive stages, the level of occupation beside the tower rose by 6.25m., and the ground level in the latest phase is level with the present height of the tower. A further 2.50m. remains to be cleared to bed-rock. In the earlier periods, the houses were not built immediately against the tower, a circulation-space, with a surface sloping rather steeply up to the tower, being left free. As the level against the tower rose, however, the buildings seem to have approached more closely. Only fragments of the plans of these later stages, however, survive, for the levels have been denuded on a steep slope from east to west, presumably in the decay period following the collapse of the top of the town wall.

To the north of the tower in Site F I, the picture is somewhat different. There was a similar steady rise in the level adjoining the tower, but in the earlier stages the buildings were actually built against the tower. The evidence that there was a succession of phases connected with the tower was described in the report on the 1956 season (*P.E.Q.* 1956, pp. 71–72). The tower in its final surviving stage had two successive skins built against an original core. The outer core, which is against a segment of the tower only, is best seen in *P.E.Q.* 1955, pl. XIII.2, and the inner one in *P.E.Q.* 1956, pl. VII. It was pointed out in the 1956 report that the inner skin was later than a very substantial stone wall—C.T.W.—parallel to, and earlier than, the great town wall, J.T.W. This wall, C.T.W., which had been reached in a sounding only, has now been further cleared. It is 1·50 m. wide, surviving to a height of 3·55 m., and is free-standing on both sides. There is a very strong presumption that it is contemporary with the original core of the tower, for its truncation by

the inner skin of the tower is clearly seen in *P.E.Q.* 1956, pl. IX.2, and, in this report, pl. XXI A and XXII A. The actual junction with the core has not, however, been cleared, and probably cannot be, for it is doubtful whether we ought to cut through the very well preserved inner skin, which is so impressive from a tourist point of view.

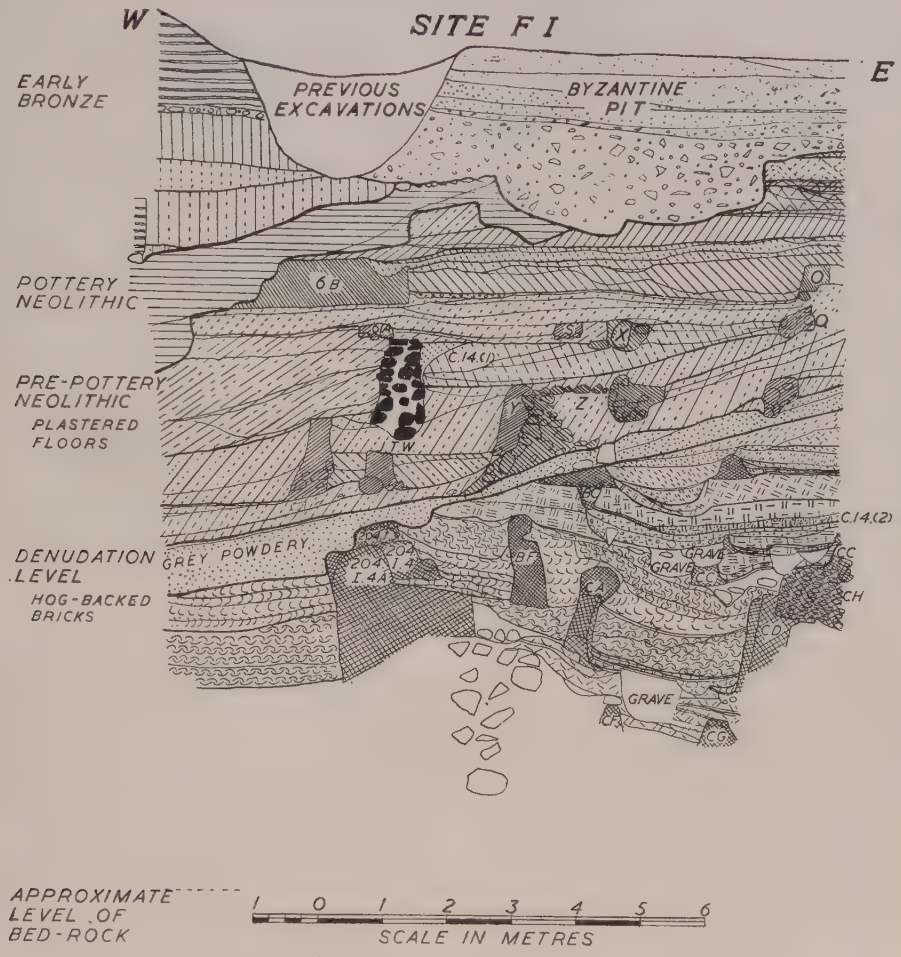


Fig. 1.

Associated with wall C.T.W. are a number of thin mud-brick walls, forming curved enclosures (pl. XXI A). They are carefully plastered, as is the inner side of C.T.W. They apparently impinge on the tower. It is possible that they are house walls, but they are quite unlike the architecture of the ordinary houses of the period; the walls are much thinner, there is a complicated series of enclosures, which are not sunk below the adjacent surface,

as are all the other houses found. No openings in them have so far been found. The problem is discussed below.

The next stage represents the filling up of these rooms with a fill consisting for the most part of very large boulders[1]. Over the buried tops of these walls, and on this fill, was built the inner skin against the tower, with a smooth, hard surface sloping away from it (pl. XXI B and XXII A). At this stage, there were no structures built against the tower. It is, however, to be presumed that a wall took the place of C.T.W., now abolished, and such a wall is in fact essential to retain the level against the tower, which is *c.* 4·50m. higher than the external level to the west. It is probable that this is represented by the lower courses of the subsequent town wall J.T.W., which, as can be seen in *P.E.Q.* 1955, pl. XIII. 1, are of a different and rougher build than the upper part.

After an interval in which some layers of wash had accumulated on the surface against the tower, a further series of enclosures was built against it, similar in character to the earlier series, with similar thin, curved, plastered walls. For the most part, these again have no openings, with the single exception of one shown in pl. XXII B, very inconveniently narrow to be a doorway, and with its base 0·35m. above the floor level. The enclosures were not built simultaneously, and three successive phases can be identified.

In the next stage, these enclosures were filled in to a height of about 1·25m., and their walls then rebuilt on approximately the same plan. This is the stage at which the outer skin was added to the tower, which, as was described in *P.E.Q.* 1956, p. 71, was contemporary with the building of the great town wall, J.T.W.

Thus at three successive phases there was a series of similar enclosures against the north side of the tower. That they do not represent ordinary houses is suggested by their difference from all other building plans of the period so far found. The virtual absence of openings is also against an interpretation as houses. The complete circuit of all the enclosures does not lie within the excavated area, but there are a number of instances of enclosures in which the walls stand to the height of about a metre, and in which there are definitely no openings. If they are not dwelling houses, they must presumably be storage chambers of some sort. The only possible pointer to an explanation is a definite channel in the surviving top of the tower (*P.E.Q.* 1955, pl. XIII. 2), leading towards them. If the staircase in the tower is associated with access to the original source of the spring, it might be that these enclosures were intended for water storage, but this hypothesis is not supported by any indication of water-laid deposits, and though the walls of the enclosures are carefully plastered, the floor levels

[1] This fill is indicated on the section, fig. 1, but clearance in it had not been carried very far on the actual line of the section. Walls CF and CG are the two walls of this phase which are visible on the left of pl. XXI A.

A. Site D. The tower with inner skin-wall, and the enclosures and wall C.T.W. which preceded it.

B. The inner skin-wall of the tower with plastered surface sloping away from it

A. The tower, cleared to inner skin-wall and contemporary surface with, on right, later town wall J.T.W., and earlier wall C.T.W.

B. The only opening in the later series of enclosures against the tower.

A. Site E. The earliest pre-pottery Neolithic so far uncovered.

B. Site E. A typical building of the earlier pre-pottery Neolithic phase.

are much less clearly defined. Perhaps further evidence on the purpose of the structures in the tower will throw more light on this problem.

As the section, fig. 1, shows, all the subsequent stages are increasingly affected by the denudation which followed the collapse of the upper part of the town wall J.T.W. Evidence however survives of a further seven structural phases.

Preceding the first of these, the final phase of the enclosures against the tower was in its turn filled in, and the subsequent buildings were of the normal type, with the sunk interior floors and all the other characteristic features. The buildings, however, tend to lie on the northern and eastern edge of the excavated area, and therefore not much of the plan was cleared. Moreover, the successive buildings partially overlie their predecessors, and the characteristic cutting-down of the floors has frequently mutilated several preceding stages; the house remains in this area are therefore less well preserved than in other areas.

There is, however, one point of very great interest. One house, which represents the tenth of the building stages so far excavated in this area (with the possible town wall C.T.W. and the accompanying enclosures and probably the first stage of the town representing the earliest so far excavated), was destroyed by fire. Only a small portion of the building lay within the excavated area, but from its floor was recovered a very large quantity of charcoal, and the remains of complete beams were visible. This charcoal has been subjected to radio-carbon analysis, and the following results have been obtained:[1]

F.40 F.40	2 different pre-treatments	6,770 B.C. ± 210 6,850 B.C. ± 210

The position of the building is indicated on the section, fig. 1, by C 14 (2). This is the first dating obtained for a level of the hog-backed brick phase. Though every caution must still be used in relying on Carbon-14 dating, the two separate tests have given very consistent results. They are, moreover, compatible with the date of 5,850 B.C. (see *P.E.Q.* 1956, p. 74) obtained from material from the plastered floor level in the same area which is indicated on the section by C 14 (1). This is separated from the level from which the earlier material is derived by four building stages of the hog-backed brick phase, the stage of the collapse of the town wall and a denudation phase covering an unknown length of time (*P.E.Q.* 1956, pp. 72–3), and then eight building stages of the plastered floor phase. It therefore seems not unreasonable, on present evidence, to assume a date in the seventh millennium B.C. for the hog-backed brick phase.

[1] The Expedition is deeply indebted to Professor F. E. Zeuner for the study of this material. It was prepared in the Environmental Archaeology Laboratory of the Institute of Archaeology and the dating was carried out by Mr. Vaughan in the Davy-Faraday Laboratory of the Royal Institution.

The remaining area in which excavation was carried out was Site E I–II–V, in Square E.7. Here, the excavations of 1956 had already traced five building stages of the hog-backed brick phase, preceding the cutting of the stream-bed described in *P.E.Q.* 1956, p.73. To this sequence seven stages were added in 1957, making a total of twelve structural stages of the hog-backed brick phase, in a total depth of soil of *c.* 3m. A sounding in the north-east corner indicates that there is a further 1·25m. to be excavated before bed-rock is reached; this sounding is to be seen on the left-hand side of pl. XXIII A, and bed-rock is just visible at its base.

Of these building stages, the two earliest so far cleared are exceptionally well preserved, both being of the usual type with sunk interiors. Of the earlier, about half of the main room would appear to be within the excavated area (pl. XXIII A). The walls are built on a curve, but the building is not circular, as two curved segments join at almost a right angle. The entrance, with steps leading down from the outside, is in the extreme angle of the excavated area, and only half was cleared. As the photograph shows, the walls of the house are well built and substantial. Adjoining the main room on the north was an area, not sunk, in which cooking and other domestic operations took place. It is covered by a spread of charcoal, a grinding stone is set in the floor, and there is the base of a possible oven. The boundary wall was destroyed by the stream-bed, but it is probable that this area was a courtyard rather than a room.

It is surprising that in a stage which should be early in the occupation of the site, house-building is already so firmly established. It remains to be seen if the 1·25m. deposit yet to be investigated will show any more tentative beginnings.

Some two-thirds of the succeeding building lay within the excavated area (pl. XXIII B). This building again is solidly built. The walls are ·75m. thick, and the interior surface is *c.* ·80m. lower than the exterior. The entrance steps are visible on the left of the photograph.

So far as the evidence goes, these structures would appear to be single-roomed. In a subsequent stage, however, a building on the extreme western edge of the excavated area would appear to have at least three rooms, an original one, of which only the edge was cleared, and two subsequently built against it.

A point of interest on which there was confirmatory evidence in all the excavated areas was the method of burial in the hog-backed brick phase. In the plastered floor phase of the pre-pottery Neolithic, burial was beneath the floors of the houses, but apparently just in the fill, or at most in very shallow graves. In the earlier phase, burial was again beneath the houses, but in definite graves about a metre deep. The bodies lie in a contracted position at the base of the graves. In an example on the line of the section shown on fig. 1, the grave was cut through early floors of the building, but sealed by the later ones. In the case of one grave, in Site D I, the grave is cut through the

latest floor, and is sealed only by the building debris; it would suggest in this instance that after the death and burial of the owner the house was intentionally demolished.

The 1957 excavations, short in duration though they were, have thus thrown much light on the earlier of the two pre-pottery Neolithic phases so far discovered. Considerable progress has been made in the elucidation of the history of the great tower and the associated defences; a number of excellent examples of houses have been cleared, and surprising evidence has been produced that the lower we go, the more solid the houses seem to be; a long sequence of structural phases has been shown to precede the level which has produced material giving a Carbon-14 dating of *c.* 6,800 B.C.

Preparations are being made for a further season of excavations starting in October 1957. This is planned as a final one before the full publication of the results of the series of excavations which started in 1952. In this it is hoped to examine the lowest levels in those areas where the pre-pottery Neolithic is under examination, and in the other areas solve the main outstanding problems in the various later periods.

THE KEPHAR BEBHAYU CONVEYANCE[1]

By S. A. Birnbaum

I. The Letters

This chapter is devoted solely to an examination of the writing, so many letters of which are mutilated and therefore difficult to establish. Our document is written in the Negeb script.[2]

In order to avoid being influenced by the contents and so arriving at erroneous readings of doubtful signs, we have confined our attention rigidly to the letters. This spade work has been shown in detail.

Line 1

The third letter is a samekh—the right-hand half of the top stroke is effaced, only its right-hand edge being visible.

The next letter, a daleth or resh, is followed by what might be either a shadow or the trace of a sign. If the latter, then it might have formed part of a gimel, hê, waw, zayin, heth, yodh or lamedh. After that there is a hole, followed by a black configuration which appears to consist of two parts: on top the lower end of a downstroke, and below this the left-hand end of

[1] Abbreviations:—

AG S. Abramson and H. L. Ginsberg: "On the Aramaic Deed of Sale of the Third Year of the Second Jewish Revolt" (*Bull. of the Am. Schools of Or. Res.* No. 136 [1954], pp. 17–19).

BKA "Bar Kokhba and Akiba". By the present writer (*PEQ* 86 [1954], pp. 23–32).

BKL Bar Kokhba Letter. See *BKA*.

BMD Beth Mashku Document. See *BMDB*.

BMDB "The Beth Mashku Document". By the present writer (*PEQ* 87 [1955], pp. 21–33).

EP Elephantine Papyri.

EPC A. Cowley: *Aramaic Papyri* (1923).

EPK E. G. Kraeling: *The Brooklyn Museum Aramaic Papyri* (1953).

HS *The Hebrew Scripts*. By the present writer (1954–).

KBC Kephar Bebhayu Conveyance.

KBK Kephar Bebhayu Marriage Deed.

KBK 1 "The Kephar Bebhayu Marriage Deed". By the present writer (*Journ. of the Am. Or. Soc.* 77 [1957]).

KBK 2 "The Place of the Kephar Bebhayu Marriage Deed". By the present writer (*Bibl. Or.* 14 [1957]).

M J. T. Milik: "Un contrat juif de l'an 134 après J.-C. (*Rev. Bibl.* 61 [1954], pp. 182–190).

NS "The Negeb Script". By the present writer (*Vet. Test.* 6 [1956], pp. 337–371).

R J. J. Rabinowitz: "Some Notes on an Aramaic Contract from the Dead Sea Region" (*Bull. of the Am. Sch. of Or. Res.*, No. 136 [1954], pp. 15 f.).

[2] See *NS* and my earlier papers in *P.E.Q.* 84 (1952), pp. 118–120, 85 (1953), pp. 23–41, and pp. 28–31 of vol. 87 (1955).

an oblique stroke, the greater part of which would have occupied what is now a gap on the right. This stroke could thus have formed part of only a hê, mem or ayin. Between this letter and the preceding resh there would not have been room for an additional letter—as suggested above (gimel, hê, waw, zayin, heth, yodh or lamedh)—but the two traces might have belonged to a single letter. Our two lists of possibles both contain hê, and that must be the required letter.

The trace above the left-hand bottom end of this hê might have been part of a daleth, zayin, yodh, lamedh or resh. Of these it would most easily fit into the shape of the two specimens of lamedh contained in the same line.

Next comes the bottom end of a possible aleph or beth. This is followed by the bottom trace of what might have been a daleth, waw, yodh, lamedh or resh. The next trace (or traces, if what we see above it is ink) might belong to a daleth, hê or resh. If it was a hê then there is not enough room for the preceding letter to be a daleth or resh—it would have been a waw, yodh or lamedh. If, on the other hand, the second letter was a daleth or resh, then its distance from the preceding letter would have been too great for the first letter to be a waw, yodh or lamedh—it would have been a daleth or resh.

The subsequent five letters are clear. At the right-hand bottom edge of the hole that follows we have the end of a stroke that might have formed part of a teth or taw. Following this is a slightly mutilated heth.

After the next four letters there is a partly effaced sign that could have been a daleth, waw, zayin, yodh or resh.

The sixth letter after this is a probable daleth or resh, followed by what is evidently the base of a beth. Next we get a mutilated beth.

The last two letters of the line differ in length and seem to stand for the sequence yodh waw.

Line 2

The first ten letters present no difficulties—apart from the daleth/resh ambiguity—in spite of the bad state of the papyrus after the fourth letter. The eleventh sign is unlike any letter of the Hebrew or the Nabataean alphabet. It would be possible to derive it from final sadhe or qoph. We meet the same sign again at the beginning of the next line, and this position precludes the reading as final sadhe, of course. Our sign might, on the other hand, be made up of two letters, the first running into the second. For the second, final nûn immediately suggests itself. To identify the first we have to look among the letters having a base or bottom stroke. The most likely seems to be a nûn. Other possibles—allowing for greater distortion—might be: beth[1], kaph, mem, samekh or pe.

[1] Cf. *NS* 340–341: 3, 5, 6, 8, 15

The third, fourth and fifth strokes from the end of the line, belong together, although the meeting point of the third and fourth strokes is effaced. The outer strokes (third and fifth) curve in opposite directions. At the point where they are nearest to each other they are linked by a short bridge (fourth stroke). This sign is the letter teth. The second stroke from the end, which touches the preceding teth and the succeeding aleph, seems to be a slightly mutilated waw or yodh.

Line 3

As to the first sign, see the discussion of the eleventh sign in l. 2.

The next five letters are followed by a trace that might belong to a gimel, hê, waw or zayin. Then comes—at the line bottom—a trace that is obviously the end of a downstroke with a leftward slant. It might have formed part of a gimel or hê. There being no room here for two letters, the two traces must belong to one and the same letter. Our two series of possibles have gimel and hê in common. The damaged letter would therefore be one of the two.

In the middle of the line—exactly below the second beth of l. 2—there is a mutilated letter of uncertain identity. It looks as if the dot just under that beth were the top of a lamedh, and the surface of the papyrus between this dot and the main stroke actually is damaged. But that main stroke has not the typical slant of a lamedh and it ends by turning rightward instead of leftward. Thus the dot is presumably after all a shadow, like that below the daleth/resh—five letters to the left. Our letter might then be a nûn of the same type as the fourth sign on the left, the base practically disappearing into the subsequent hê. Further to the left there is another nûn whose base also touches a subsequent letter.

Line 4

Above the sixth letter (heth) there appears to be a spot of ink that could hardly have formed part of a letter. In the tear between the heth and the slightly mutilated sadhe there is room for a narrow letter: waw, zayin, yodh or lamedh; there may, however, have been a blank space.

Line 5

The seventh sign—the letter after the aleph—is a mutilated waw or yodh. This sign is followed by a (not quite complete) waw or yodh. The third sign after this has an unnaturally straight top edge suggesting damage by flaking off. The blackness above it appears to be shadows although the stroke seems to have run on upwards into it. If so, the letter would be an aleph.

The sign after the gimel in the middle of the line is straight. If it were a zayin, we would expect the characteristic rightward top; moreover, a

typical zayin slants in the opposite direction, or is at least vertical. Thus a waw or yodh is, on the whole, more likely.

The last letter of the line is puzzling. The left-hand side looks like a final pe with its left-hand stroke curving back into the main stroke. The right-hand part of the whole configuration would then be an independent letter, running into the final pe. It does not, however, look like any of the letters of this MS. The nearest resemblance is with the right half of a mem, samekh or pe. But where then would the left half be? Was it run into the pe and swallowed up by it? The letter could not, in that case, have been a mem. If the first letter were a pe it would be possible because only the bottom tip of its left-hand stroke would then be covered by the main stroke of the subsequent pe. But if the first letter were a samekh, its left-hand stroke would be too big to disappear into the final pe. In that case *sp* would be a monogram: the left-hand stroke of the samekh serving at the same time as the upper part of the long downstroke of the pe.[1] It might be tempting to explain this as a cursive development, thus: the bottom stroke of the right-hand letter was produced to the left and then bent upwards to form the left-hand stroke of the final pe; on reaching the line ceiling the pen turned downwards and the resulting stroke served for both letters. Upon careful inspection this explanation is, however, not acceptable, for this reason: there is not one continuous bottom stroke but one on the right and one on the left of the main stroke: the bottom stroke of the right-hand letter and the bottom part of the left-hand stroke of the final pe. We must therefore assume that the left-hand stroke of the final pe came down from the top in the normal way. This monogram was accordingly made with four pen lifts. (Was the writer perhaps rendering an existing one-lift type with four pen-lifts?)

Line 6

The gap after the eighth letter (hê) is followed by an unusual curve. It does not look like part of any of the letters in this MS. Further inspection shows that the right-hand tip of this curve differs clearly in tone from the downstroke and might not belong to it at all. Perhaps this is corroborated by the appearance of the bottom end of the downstroke. The abruptness with which it stops might indicate damage through flaking off. The letter seems to have been a waw or yodh.

The tiny right-hand tip mentioned might have belonged to a lamedh or mem. Below it, somewhat to the right, there is another spot of ink: it might have formed the extremity of a gimel, hê, heth, mem, nûn or ayin. There is no room here for two letters. Can both traces be fitted into any single letter, common to our two series of possibles? Yes, into the mem: the lower

[1] See *NS* 340, especially the samekh of No. 6.

trace is the bottom end of the oval, the upper one is the end of the tail.

The stroke after the taw might have been the right-hand half of a daleth, mem, samekh, pe or resh. On the photo it is impossible to make out how much of the black configuration following it is ink and how much shadow. The biggish triangle at the end seems to me to be a shadow and the two dots before it, ink. If they are, they might well belong to the tail of a mem.

The next sign, though slightly effaced, is a nûn.

The letter between ayin and final nûn, in the middle of the line, resembles the daleth or resh in the *KBK* (and resh elsewhere) but it differs markedly from the specimens of these letters contained in our MS. Perhaps it is closer to a zayin, except that the top is vertical, not oblique as in a zayin here. Can it be a slightly distorted waw or yodh?

Before the shîn towards the end of the line, between the lamedh and daleth/resh, the writer has squeezed in on top a small mem which he had accidentally omitted (*M*).

Line 7

The seventh letter is damaged on the right-hand side. There is room for only a plain-stroke letter here. It could be either a waw of the straight variety discussed in l. 5 (if that was a waw) or a zayin.

The trace before the large hole is hardly identifiable. It may have belonged to a beth like that of *zb* in the second half of l. 5. Hê, teth, mem, samekh, pe and taw are other possibles—none of them attractive but not out of the question either.

In the ensuing gap there would have been room for two or three letters.

The little line which follows the hole at, and above, the line ceiling appears to be a shadow.

The trace after the hole might represent the top of a daleth, hê or resh. The smallness of the left-hand stroke and the slant of the right-hand one speak against daleth and resh—the right stroke here slopes rightward while in daleth and resh it slants initially to the left. Furthermore, in our sign the left-hand stroke seems finally to turn downwards. It stops abruptly with an unnaturally straight bottom edge, suggesting that the stroke might not have ended there, i.e., that the rest has flaked off—the big tear at this point would have affected the writing on the edges by damaging the surface of the papyrus there. For these reasons it is more likely that the trace formed part of a hê.

Line 8

The traces of the first letter seem to fit in with the shape of an ayin. Next we have the top (mutilated) and the bottom of a pe, like that of l. 4, letter 8. The third sign might be daleth, waw, yodh or resh. The fifth sign might be the lower part of a daleth, waw, yodh, lamedh or resh.

Where the big hole begins there is a tiny trace which might have

belonged to an aleph, daleth, zayin, heth, kaph, final kaph, nûn, final nûn, ayin, qoph or resh.

The hole has obliterated seven to eight letters. Then there is a dot, just under l. 7; at this point it touches the end of the gimel. This can only be the top of a lamedh. The next trace might be the top of a beth, gimel, daleth, hê, waw, zayin, yodh, nûn, final nûn, samekh, ayin, resh, shîn or taw. What follows is a daleth or resh.

Line 9

The first sign must be one of the narrow letters: waw, zayin, yodh or lamedh. If it were a lamedh, the straight black line just above could not be the top stroke of that latter because its direction and position preclude the possibility of its being a continuation of the bottom stroke. It is a shadow.

The second letter is clearly a hê. The third might be a waw, zayin, nûn or final nûn.

As regards the fourth letter, the papyrus fragments do not seem to have been placed in quite the correct position here: the outline on the left has a break in the middle which seems to indicate that the top and bottom parts do not belong to each other. The bottom part ought perhaps to be moved slightly leftwards to form the bottom of the subsequent (fifth) letter. Our sign would then be an aleph, mutilated at the lower end.

The next one is clearly a damaged shîn, followed by a stroke belonging to a possible daleth, waw, zayin, yodh, lamedh, nûn or resh; other letters which might have ranked as possibles are excluded by the narrowness of the space. The next letters are ayin and waw.

The second sign under the hole does not look like any of the letters in this MS.: the strokes of none of the other letters cross. This might indicate that the two strokes do not form a single letter. The second stroke would be part of a final letter—kaph, nûn or pe. In the previous line, in discussing the trace above our strokes, one of the possibilities mentioned for that trace was a final kaph. Our stroke here in l. 9 seems to confirm this. The letter would then be an exceedingly long specimen.

Of the two strokes that cross, the right-hand one cannot form part of a hê because it turns vertical at the top. It is too long and low for the tail of a mem. Perhaps it is an ayin; the top bit would then be the end of the left stroke of that ayin.

Under the left-hand end of the hole, between the mem and beth, the lower extremity of a narrow letter is visible. It might have belonged to a waw, zayin or yodh; aleph or lamedh are not precluded. The sign after the beth seems to be a waw or yodh; a lamedh, too, is possible.

Line 10

The eighth letter appears to be a mem rather than a samekh. Its

left-hand side is mutilated, and we cannot tell whether it was followed by another letter. Next we have parts of a beth: the downstroke running to the left-hand end of the base; a trace of the top is faintly visible.

After the hole, in the middle of the line, fragments of a letter are preserved: a downstroke with a leftward slant and a smaller stroke—apparently issuing from it—which runs down vertically. These traces fit in perfectly with a mem: the oblique stroke is the end of the tail, the lower trace is a bit of the left-hand side of the letter.

After the ayin that follows we get a very long stroke running right up to the line above and touching a letter (aleph) there. In spite of its unusual length it looks like a single stroke and not as if it were partly a shadow caused by a break in the papyrus. It would be a lamedh, of course. The continuation of this line downwards—from about the upper third of the ayin to the line bottom of the next line, however, looks on the photo like a shadow cast onto the background by a break in the papyrus.

The strokes after the next hole do not constitute a sadhe: there is a short top stroke on the left which precludes that reading. What we have here is a pe preceded by the end of a missing letter. This oblique little stroke must be the tail of a mem.

Line 11

Enough is preserved of what, according to its position, is presumably the second letter to identify it as a waw or yodh.

The sixth sign before the hole is a hê, carelessly written—the left-hand stroke does not issue from the right-hand top corner, as in the sequence *drth* in l. 5, and it is not oblique.

The long, oblique stroke that runs down from the left-hand top corner is puzzling. Is it just a slip? Or is only the upper half a slip, while what appears to be the lower half is really the lower half of the subsequent stroke, i.e., have we a final nûn here? There are similar forms on this papyrus but none quite so curved as this specimen would be. It therefore seems, on the whole, more likely that no part of the long line belongs to a letter.

At the beginning of the big hole there is a trace of a letter that might be a daleth, hê, waw, heth, yodh, kaph, lamedh, mem, samekh, pe, resh or taw. If it were a lamedh, the stroke immediately above (in l. 10) might belong to it.

After the hole there is the end of a letter base. As it is concave the sign is more likely to have been a beth than a kaph, nûn or pe.

In the gap there would be room for one to three letters.

Line 12

At the right-hand edge of the big hole there is a black configuration.

The upper stroke could belong to an aleph, ayin or shîn. If it were an ayin, then, at that height, its left-hand stroke should already be visible; if

the letter were a shîn then the stroke in question would, according to its height, be the inner stroke, and the parallel stroke on the right should be visible. Hence the upper stroke is likely to have formed part of an aleph.

The lower part of the black configuration, at first glance, appears to be a shadow. If it were, its rectangular outline on the right—which would then coincide with the edge of the papyrus—would cast a rectangular shadow to the left. Instead of this we have a straight line on the left. The lower part is therefore a stroke (apart from the extreme left-hand bottom tip). It could form part of an aleph or, less likely, of a gimel or hê. As the upper part of the configuration belongs to an aleph there can be no doubt that the lower part is its continuation.

After this, at the bottom edge of the hole, we have a very small oblique stroke. It might be a base—one that turned out somewhat too thin and slanting—or else it might be the end of a curving downstroke. Possible letters would accordingly be: beth, gimel, hê, teth, nûn, ayin, pe, sadhe, shîn, taw. The hole could accommodate three to five letters.

The first letter after the hole is difficult. A shorter downstroke on the right and a longer one on the left point to a taw—but the strokes are straight, in contrast with the pronounced curves of a typical taw; nor is there a loop. Two parallel straightish strokes would be characteristic of heth, except that in heth the position of the shorter and the longer stroke is the reverse of what we have here.

Line 13

The third stroke might belong to a gimel, waw or yodh. The next trace is clearly the upper part of a lamedh. Left of it there is a tiny grey line slanting down to the left. Is it part of a letter? If so, it might have been the extremity of an ayin or shîn, either of which letters would then have filled the gap. Next we have the upper part of a lamedh again.

The stance of the third letter after that would favour identification with a zayin rather than with a waw or yodh.

The trace on top of the next hole presents difficulties. It does not really tally with any of the letter tops, and all we can do is to say that the following letters are not quite impossible: daleth, hê, waw, zayin, yodh, kaph, nûn, final nûn, resh. Diagonally opposite it on the left, on the lower edge of the hole, there is a trace which might conceivably form part of the same letter, giving us a possible hê or kaph. On the other hand, it might belong to the next letter—although this would then be rather wide—i.e., it would be the end of the right-hand stroke of a taw; the left-hand half of a taw is clearly preserved.

After the taw comes the top of a letter which is hardly identifiable—perhaps it was a daleth, waw, zayin, yodh, nûn, sadhe or resh.

The traces after the big hole might have belonged to a hê, mem, ayin or taw. Of these the first three are less likely than the fourth in view of the heavy oblique stroke. It fits in better with the loop of a taw.

Towards the end of the line, the sequence *hl* seems to be followed by either waw or yodh. After this there appears to be another waw or yodh; it is also possible that both traces together formed a heth. On the other hand, the second trace might have constituted the right-hand side of a mem; the oblique stroke at the bottom would then be the tail of the mem. Alternatively, this oblique stroke might have formed part of the bottom of a beth. There is, however, no trace of its upper part. Nor is there any sign of the left-hand stroke of a mem.

Line 14

At the bottom edge of the initial hole there seems to be the remnant of the base of what would have been the third letter of the line. Letters with a base are: beth, kaph, pe and sadhe. Of the fourth letter in the line, three traces are preserved: two bottom ends of downstrokes and a straight horizontal top stroke. All these traces would fit into a hê.

The black formation at the next hole presents great difficulties. The top part looks like ink and the lower part like a shadow. What we have here might then be the top of a beth, mem or pe.

After *sw/sy* comes a teth like the one in l. 2, but the lower part of the right-hand stroke has flaked off.

What follows now is a mutilated taw: the loop has gone (the black line at the bottom is a shadow). The sign after it is very badly damaged. The right-hand downstroke and base suggest a mem like the one in l. 13 (middle).

The next letter (beth) is followed by a sign which can only be a mutilated gimel: the upper half of the main stroke has come off.

The rest of the line is written by another hand, with a different pen.

After the big hole, there is the left-hand part of a taw, and perhaps the bottom tip of the right-hand stroke.

Line 15

The first trace might be the top extremity of an aleph, daleth, zayin, heth, kaph, nûn, ayin, resh or shîn. The second trace might be the bottom end of a daleth, waw, zayin, heth (one of the downstrokes), yodh, lamedh, samekh (left-hand stroke) or resh. A third possibility is that the two traces formed part of one letter. If so daleth, zayin, heth or resh, which occur in both series, are at our disposal. In view of the position of the two traces, only heth would satisfy the requirements.

If, on the other hand, the second trace were to be connected with the third and fourth ones, the sign could only be a hê. But if only the third and fourth traces belong together, they would constitute a daleth or resh.

The letter after that is clearly a daleth or resh: above the hole the tips of the main stroke and of the top stroke are showing and under it the bottom end of the main stroke can be seen. If the preceding letter is also a daleth

or resh, then the writer apparently took the trouble to differentiate between the two letters because they followed each other.

The bottom stroke of the next—mutilated—letter reveals it to be a beth. The next stroke might belong to a daleth, waw, zayin, heth, yodh, lamedh or resh.

The next trace might be the bottom end of a daleth, waw, zayin, heth, yodh, lamedh or resh.

Next, after the little hole, are two downstrokes which might form part of a hê. Whether the oblique little stroke above could be the top stroke of such a hê is very doubtful.

The trace after the next hole might belong to the top of a daleth, kaph, ayin or resh.

The oblique stroke before the hole in the middle of the line might form part of a gimel or hê.

The rest of the line is written by a different—a third—hand.

After the second letter (lamedh) there is a tiny dot which might be an ink trace. It is, however, too high up to form part of a letter. The sign after the next hole might be a daleth, hê (the left part), waw, yodh, ayin (the left half), resh or taw. Daleth, waw, yodh and resh are less likely because the downstroke is too oblique. The top stroke would be uncharacteristic for a hê and the lower stroke somewhat too low down. Our sign differs clearly from the undoubted hê at the end of the line. Ayin, therefore seems to fit in best here. The right part of the letter would just fill the space covered by the hole.

The next sign might be a waw, zayin or yodh. It is followed by a daleth, waw, yodh or resh.

At the left-hand corners of the hole that follows, there are traces of a letter. They are compatible with daleth, waw, yodh, kaph, samekh, ayin or resh.

The letter after the first mem is a shîn, on the general pattern of *NS* 17a, 18, 22.

The next sign is difficult. The manner of construction suggests a teth but the main strokes are straight instead of curved; the right-hand stroke starts at the line ceiling instead of half-way down. Identification as a taw would be open to even stronger objections: the left-hand stroke stops less than half-way down, and there is no loop; the right-hand strokes are completely straight instead of curved; and some of the minor features are different, too.

Nor is it feasible to split up the strokes of this sign: only the left-hand one could constitute a separate letter. On the other hand, neither of the strokes could form a letter with the strokes adjoining them on the right or left, respectively.

The next letter is a final aleph, the lower half of its main stroke being almost effaced.

The formation between the two mems that follow is misleading: the right-hand part is the end of the final nûn from the line above. The left-hand part seems to be non-final aleph, of the type *NS* 17a. But if it were, then the first letter of this second half of the line could not also be an aleph—and its identity seems beyond doubt. Could our sign be a waw or yodh? Or is its left-hand part by any chance the curled end of the final nûn? That seems very improbable. Or is there perhaps no letter at all between the two mems but just a smudge and/or shadow? The need for avoiding the end of the final nûn would explain the unusual distance between the two.

Line 16

Here we have another hand again—the fourth.

The first letter is a shîn of the type *NS* 17, 18, 22. The eighth sign might be a waw, yodh, nûn (with base effaced) or pe (the same). Then, after the hê, there might be a waw, zayin or yodh.

The general outline of the next letter would only be compatible with mem or samekh. It differs clearly from the undoubted mem which is the second letter in this line. Presumably it is a samekh. This is confirmed by another open form in our material.[1]

Line 17

This line is written by a fifth hand. The first stroke is a waw, zayin or yodh, the second a waw or yodh. This is followed by the right-hand stroke and the middle one of a shîn. Of the next letter a small trace is preserved: it might belong to a daleth, ayin or resh. The black line after that seems to be a shadow.

The next two signs are in Hebrew script. We came across such a mixture before—in the signatures of the *BMD*.[2]

After that nothing can be made out for the space of two or three letters. Then comes an—angularized—oval sign. As there is no tail the letter is, presumably, not a mem but a samekh.

It is followed by two dots which reveal themselves to be a single stroke—the middle having flaked off—because there is clearly the continuation under the hole in the shape of an oblique stroke. Thus the letter is either a final nûn or a final pe.

If the black lines after the hê are not a shadow they would make a daleth or resh.

Line 18

This line is written by a sixth hand.

The first letter (under the beth of l. 17) is a blurred hê. It is followed by a waw, zayin or yodh, and then by a daleth or resh.

[1] *NS* 20.

[2] *BMDB*, pp. 28–31.

The letter after the beth is Hebrew again, and so is the hê between the two waws/yodhs which follow, as well as the letter before the last one of the line. But the hê between the daleth and the shîn is not.

Temporary Transliteration

The foregoing examination of the individual signs and traces yields the following transliteration of our document. The number of different possibilities for many of the traces is so great that it would not have been feasible to arrange them vertically here, as is usually done where there is a choice between two or, at most, three letters only. The letters in question are therefore arranged horizontally and separated by oblique strokes. The result of so large a number of uncertainties is that coherent reading at this stage is out of the question, the more so as we have followed the arrangement of the original, where there is no separation of words.

It is hardly necessary to point out that, in spite of this, we cannot say that the writer was intending to write *scriptio continua*. The fact that final forms of the letters kaph, nûn and pe are employed shows that he was fully conscious that words are units, and that as such they are separately written. Although he wrote carelessly he must have been aware that in more careful MSS. there is a space between each word and the next—he must at some time have seen letters like the (practically contemporaneous) *BKL*, *BMD* and literary hands like the Murabb'ât Exodus Fragment.[1]

This transliteration will form the basis for the discussion in Ch. II.

1 בעסד/רהלא/בו/י/להשנתתלט/תלחד/רתו/ישד/ו/ז/י/ראלבכפד/רבבו/יו/י
ד/ר ד/ר

2 חד/רד/רבד/רו/יהו/יד/רהק/בן/כן/מן/נן/סן/פן/כפד/רבבו/יו/יאמד/רלאלעז
ד/רבד/ראלעזד/רשטו/יא

3 ק/בן/כן/מן/נן/סן/פן/תמזאנג/הק/בן/כן/מן/נן/סן/פד/רעו/יתו/יו/יו/ימאד/רנהז
בנתלדו/ימהד/רנהלבתהד/רו/ילו/י

4 ד/רו/יפתו/יח[ו/ז/י/ל/ס]צפולגהד/רד/רתו/יד/רו/יתפתחנהלגהבתך

5 ו/יד/רשהלאו/יו/יתו/ילדלדעמו/יבנו/יד/רד/רתהד/רדזבנתלדבבס/פף

6 זו/יזו/יך/רו/יהמו/יתד/מ/ס/פ/רנו/יהסלעו/יתד/רתיד/רו/יגמד/רו/יולעלמ
ד/רשו/י

7 אלעזד/רבו/זבתהד/ו/י/רדאבנו/יאשד/רו/יב/ה/ט/מ/ס/פ/ת[– – (–)]הגד/ר
ו/יאבו/ילד/רו/יבה

[1] *HS* No. 89.

8 עפד/ו/י/רהד/ו/י/ל/רקד/ר/קעאתחמאבתהד/ד/ר[א/ד/ז/ח/כ/ד/נ/ן/ע/ק/ר
- - - - - - (-)]ל[-]ב/נ/ד/ה/ו/ז/י/נ/ן/ס/ע/ר/ש/תד/רזבנהמד/רנחא

9 ו/ז/י/להו/ז/נ/ואת[-]שד/ו/ז/י/ל/נ/רעו/יוצפנהד/רד/רתהמעד/רבא[-]ד/רד/
רמא/ו/ז/י/לבו/ז/י/להו/יד/רשהלא

10 או/יתו/ילדעמ÷בנו/יד/רד/רתהד/רו/ילד/ו/י/ך/רו/ילא[-]מעלו/ימפקעלד/
ו/יראנתהחד/רד/ר

11 [-]ו/ימהד/רנהו/ילעלמו/יאנהאהו/יו/יעד/רב[- - (-)(-)]בנבתהד/רדבן/נד
/ו/י/רמהד/רנה

12 ו/ז/י/ללעלמו/ז/יאנהשלמבד/רתשמעו/יקא/ב/ג/ה/ט/נ/ע/פ/צ/ש/ת
[- -(-)(-)]ח/תד/רד/רד/רנהמלו/יקלאו/יתו/י

13 לו/יג/ו/ילע/שלמבזבנבתהד/ו/י/רד/רו/ילעלמד/ה/ו/יכ/נ/ז/רתד/ו/ז/י/נ/צ/ר
ה/כ

[-(-)(-)]תהו/יד/רו/ינקנהל { ו/יו/י / ח / ו/ימ / ו/יו/יב } ל-

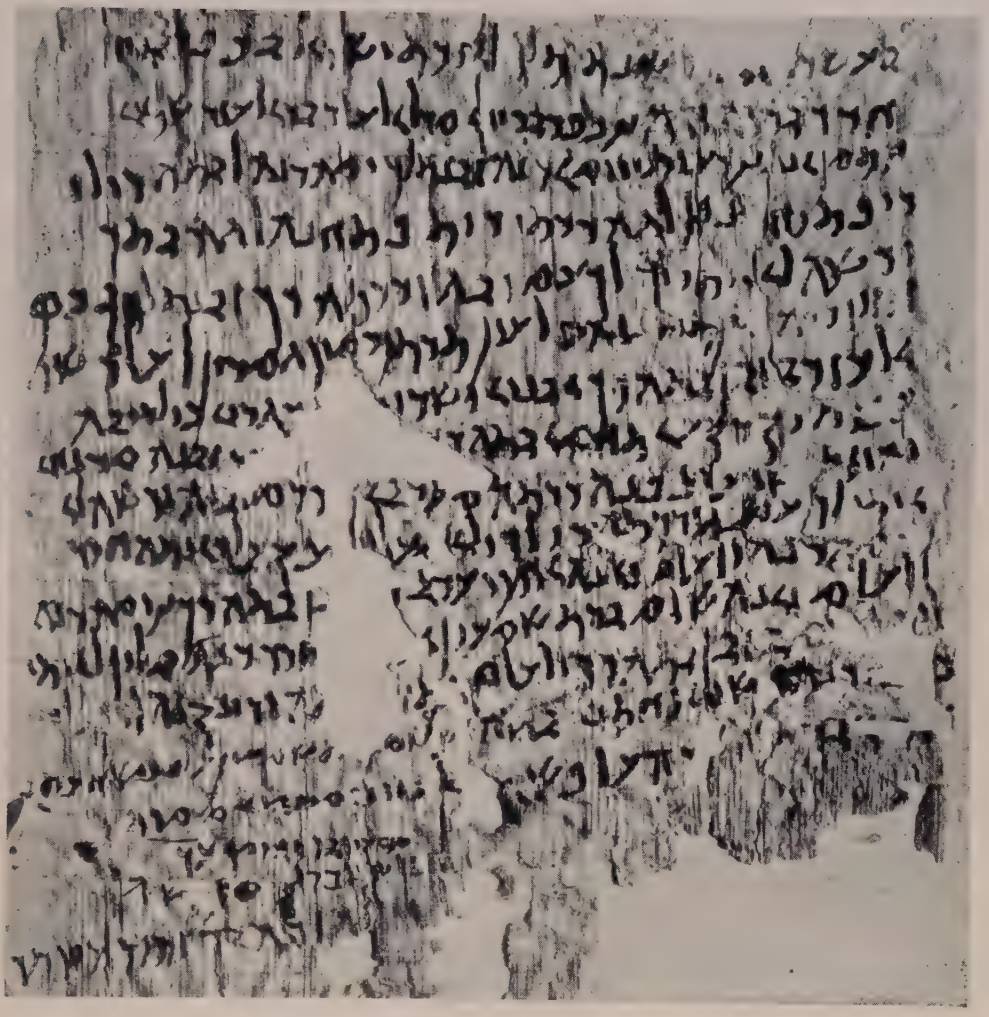
14 כ[-]ב/כ/פ/צהד/רנהב/מ/פשו/יטו/יחתמבגו/יה[(-)]שלו/ימ[- -]תשמעו/יקע
לנפשהכתב

15 { א/ד/ז/ח/כ/נ/ע/ר/שד/ו/ז/ח/י/ל/ס/רד/ר / חד/רד/ר / א/ד/ז/ח/כ/נ/ע/ר/שהדר } ד/רבד/ו/ז/ח/י/ל/רד/ו/ז/ח/י/
ל/רה[-]ד/ כ/ע/רהעלפשנ/ה[(-)]אל/עו/ז/יד/ו/י/ר[-]ד / ו/י/כ/ס/ע/רמשטא
מימד/רה

16 שמעו/יקבד/רו/י/נ/פהו/ז/יסקעך

17 ו/ז/יו/ישד/ע/רבד/ר - -(-)סן/ק[-]שהד?/ר?

18 הו/ז/יד/רהבד/רו/ז/יהד/ו/י/רד/רהשהד/ר



The Kephar Bebhayu Conveyance

II. The Words

Line 1

There is no difficulty in isolating the first word which is clearly *b'srh*. Our MS. is obviously not a column from a book but just a single sheet. As such it would be a letter, a legal document, or the like. A glance at the end tells us that there are a number of what seem to be signatures appended. This suggests a legal document. If that is correct, *'srh* is the numeral and designates the date, i.e., the day of the month.

The next word should reveal the month—either the name of the month, as in the *EP*, or the word *month* (*yrḥ*, later *ḥwdš*) followed by its name, as in the medieval deeds. The reading lamedh (see Ch. I) for the initial letter is suitable because the preposition *l* is required.

The next trace of a letter—part of an aleph or beth (see Ch. I)—must belong to an aleph, since no name of a month begins with a beth (nor do the words *yrḥ* or *ḥwdš*). Hence we have a choice between *'b*, *'dr*, *'yr* and *'lwl*. Our readings in Ch. I eliminate the first and last of these, leaving us with a choice between *'dr* and *'yr*. The traces appear rather to favour *'dr*.

In the rest of the line there is no difficulty in isolating the words.

We would expect the year to be followed by an indication as to the era. The next two words (*lḥrt yśr'l*) are obviously identical with the legend on some of the coins from the second year of the Second Revolt.[1] That gives us the era of our document—it was written during the Bar Kokhba War (*M*). We need hardly add that this date is confirmed by the evidence of the *BKL* and *BMD* which were found together with our MS.

The era should be followed by the name of the place where the document was penned. The next sequence of letters fits in with this requirement, *kpr* being a geographical designation. The word after that should be the name itself. This seems to be *bbyw*, recalling the sequence *b b w/y w/y* in the *KBK*, which was found together with our MS.

Line 2

After the place-name we would expect the name of the party making out the document. It can easily be isolated. The daleth/resh ambiguity, unfortunately, prevents us from establishing it with certainty.[2]

The following readings (see Ch. I) of the puzzling sign after *yhwdh* would make no sense: *q*, *kn*, *nn*, *sn*, *pn*. That leaves us with the choice between *bn* (i.e., *bên*) and *mn* (i.e., *min*).

The next group of letters—*kprbbyw*—is easy to isolate since it is identical with the end of l. 1, confirming that reading as well as the one in the *KBK* (l. 4). The ending *w* shows the name to have an Arabic form. That, in its archaeological and historical context, would mean Nabataean.

[1] *š*[*nt*] *b lḥr*(*w*)*t yśr'l*.

[2] We adopt Ḥdr (*M*).

The geographical designation disposes of the reading *bên* which could hardly connect a personal with a place name. *Mn* is the obvious choice and the derivation of the sign then becomes evident: the tick which forms the right-hand part is just a very open stage of a mem—which letter is anyhow very frequently open; the tail has been merged with the final nûn, the top of the letter being dragged down by the mem tail so that this can run into the nûn.

Next we would expect the name of the person on whose behalf the document has been made out. The word *'mr* tells us that he is being addressed by Ḥdr.

The final word of the line is baffling. As it looks like a nominal form and follows the word Eleazar, *šty'* might qualify that name. But what was its meaning? We can hardly assume that the man was addressed as " the fool ".

Line 3

The first sign, though slightly damaged, is clearly identical with what we found to be *mn* = *min* in l. 2. The same meaning fits in perfectly well here, too. There we had Ḥdr's address, *mn kpr bbyw*, and here Eleazar's, *mn tmn*. It should be noted that in *tmn* the sequence *mn* is written out in full. In other words, the ligature *mn* is used only for the preposition *min*—it is, so to speak, a kind of ideogram.

After the date and the parties, we get the subject matter of the document. It consists of a declaration by Ḥdr and is in direct speech.

It begins with two adverbial phrases. The first—*mn r'wty*—tells us that the speaker is acting of his own free will. This clause does not occur in the *EP* but it is frequently to be met with in later Jewish deeds, and is reported[1] to be present in a Hebrew deed found at Murabba'ât.

The object of *zbnt* is, of course, *bth*; *lk* must, accordingly, be the dative. Hence *zbnt* cannot mean " to buy " but must be the pa"el: " to sell ".

Line 4

It seems unlikely that *ṣpn* is directly governed by *ptyḥ*; the two words were probably linked by a preposition. There is room where the hole is, for a lamedh.

That the house is " open " to the courtyard presumably means that the house is entered via the courtyard. This idiom is not used in the *EP*.

Eleazar acquires the right to " open " the newly bought house into his old house, i.e., he evidently gains direct access from his old house to his new one. From this it would appear that these two houses were adjacent, otherwise Ḥdr would have said *tptḥnh lgh drtk* or the like.

[1] *M.*

Line 5

Lk lk is an obvious error.

Seeing that the statement at the end of the preceding line and at the beginning of this one is a negative one and refers to the 2nd pers. (*lk* = Eleazar), it is evident that the waw/yodh after *'m* is the suffix of the 1st pers., i.e., that it refers to Ḥdr.

Line 6

The selection of the correct letters from the possibles (see Ch. I) presents no difficulties. The text divides easily into words but they are incoherent: " zuz which are eight two sela ". The writer obviously forgot " eight " and added it after he had already written " which are ". What he intended was *zwzyn tmnyh dy hmwn sl'yn trtn* (*AG*).

Line 7

If we take the trace just before the hole to be the top right-hand corner of a taw, giving the reading *śryt'* (*M*), the final aleph of this word, followed by a waw, would just fill the gap.

Our reading of the next word as *hgdy'*/*hgry'* is compatible with the translation " bricks " (*AG*, reading *'gry'*) because alternation between the glottal stop and fricative is not rare. Such alternation occurs in another Murabba'ât document, the *BKL*, where *'ṣl* " by, with " is written *ḥṣl*.[1]

Line 8

The letters suggested by us in Ch. I for the beginning of the line add up to form *'prh* " dust ". The expression *' prh dqrq* " is, of course, a parallel to the formulae in the *EP* and the Babylonian Talmud: *mn 'r" w'd 'l'*[2] and *mthwm w'd rwm rqy"*.[3] Our document is closer to the Talmudic form in that it adds a qualification to " ground " (although there is a great difference between a matter-of-fact expression like " dust of the ground " and the more imaginative and theoretical wording of the Talmud: " abyss of the earth ").

But where, in this deed, is the sky part, corresponding to *w'd 'l'* or *w'd rwm rqy"*? Has it been just accidentally omitted? There seems to be rather a lot left out for that to have happened, besides which there would have been omissions in two different parts of the same sentence (a bit of the beginning: *mn*, as well as the whole second half: *w'd rwm' drqy"* or *w'd 'l'*). Or did the writer, by any chance, accidentally reverse the formula in his mind, intending to write *mn rwm' drqy" w'd 'prh dqrq"*? If so, he would have omitted only the first half.

[1] See *BKA*, p. 29, where, in addition, examples from the Qumrân MSS. are adduced.

[2] *EPC* No. 5; but the other *EP* do not have it.

[3] *B. Babha Bathra* 63 b.

After the general description of the house, its location is dealt with: the boundaries in all directions are defined.

The gap in the line, caused by the big hole on the left, would accommodate seven to eight letters, i.e., two to three words. These, no doubt, referred to the previous *bth*, while being, at the same time, connected with *zbnh* which follows. The link between the house and the transaction is, of course, the seller or buyer. Accordingly, we would expect the missing words to contain the name of either Ḥdr or Eleazar. The trace of a lamedh top is clearly visible giving us the name of Eleazar, i.e., of the buyer. That would mean that Ḥdr, who is the speaker in this deed, and who has, until now, employed the pronoun (" thou ", " thy ") in addressing Eleazar, is here using his name.

Zbnh, referring to Eleazar, must therefore be in the pe'al (" to buy "). *Zbn* is, accordingly, either the 3rd pers. pf. sg. or the participle.

Everywhere in the body of the deed Ḥdr addresses Eleazar. This corresponds grammatically to the 1st–2nd pers. relationship. Hence *zbnh* would not be the 3rd pers. but the ptc.

Accordingly, *'l'zr* would not be the subject of the passage. That subject must have been the pers. pron., 2nd pers. sg., *'nt* (*'l'zr* being in apposition to it).

'nt helps to fill the available space: the word *'l'zr* would not fill the gap —the longest in the MS. Even *'nt 'l'zr* would hardly be long enough. (The form *'nt* is almost certain in view of l. 10).

As the passage is a relative clause qualifying *bth*, it would be introduced by the *nota relationis*. And even this *dy*, as tests show, might still not quite fill the space. Hence the daleth after *bth* would presumably not be the first letter of *dy* but of another word. As a matter of fact, yodh was not one of the letters in our series of possible readings (see Ch. I). In ll.7, 11 and 13, *bth* is followed by *dk*. We have therefore probably the same word here—the surviving trace after daleth being compatible with a final kaph.

Line 9

After the heading—*mdnḥ'*—at the end of the preceding line, the new line starts with the boundary for this side (east). The text obviously goes up to the next heading (*ṣpnh*), but what it contains is very uncertain. There are eight letters (or nine, if there was one in the hole) and that would make two to three words.

According to our readings in Ch. I, the first word might be *whw*, *why*, *whn* or *yhw'*. If it is *yhw'*, the next five letters can hardly form more than one word as it is necessarily a noun. However, five letters would only be possible if there was an ending, or if the noun was in the *tp'wl* (or *tp'yl*) form. An ending is excluded because the fourth letter is an ayin, i.e., would belong to the root. The *tp'wl* (or *tp'yl*) form would be feasible. The roots with *ṣ'* as the second and third radicals are: *pṣ'*, *rṣ'* and *tṣ'*. None of these makes

sense here. In addition, the use of *hw'* would be very unidiomatic: " in the east will be (this or that thing) "—apart from the fact that *hw'* is not used in describing the other boundaries here; nor does it appear in the *EP*.

To start the line with " and " (*whw*, *why*, *whn*) is also difficult but perhaps not impossible. *Whn* " and if " would make no sense here. *Whw* might introduce the noun giving the boundary.

The next word would begin with *'t*. The only possible root is *'tr* (since *'th* is a verb), and " place " would be a suitable word here.

For the last word we would have a choice between the roots *šw'*, *šr'* and *śr'*.

Šûa' means " to plaster " and our word might then be *šy'*, the pass. ptc. It would qualify the preceding *'t*[*r*]: " plastered " or perhaps " smooth ".

Šr' means " to slope, glide ", and there is a Talmudic noun *šr't'* " slope, sloping ground ". We would then have *'t*[*r*] *šr'* " sloping place "—a possible description here, and perhaps a more likely one than " plastered " or " smooth ".[1]

Śr' conveys the idea of " joining, adding ", perhaps it means " adjoining " here. However, *'t*[*r*] *śr'* " an adjoining place " makes no sense as a boundary.

In all three cases we would perhaps rather expect the stat. determ., *'trh šy'h/šr'h/śr'h*. But there is no room for two letters (*rh* or *r'*) between the taw and the shîn, and there is no hê or aleph after the ayin. However, the stat. abs. is not impossible.

If all these suggestions should be beside the mark, the next letter, waw/yodh, might perhaps form a unit with the preceding letters, and so give us the ending *û* or *î*. Both of these are, however, generally abstract (especially the words in *û*). Or else, the waw would go with the succeeding letters, i.e., it would be the word " and ".

The gap after *m'rb'* presumably contained *w* " and ".

The western and southern boundaries of the house are formed by *bwh/bzh/byh/blh*.[2]

Line 10

The initial clause is a repetition of the one in the middle of l. 5. That answers the question (see Ch. I) as to whether there was a letter between mem and beth. The reading must be *'my*.

[1] Arabic *šr'* has among its verbal meanings that of " communicating with the road ", and among its nominal ones " situate upon a road that is a thoroughfare ", " main road ", " open track or way ". That would seem suitable but it is rendered unlikely by the fact that we would expect *sr'* (with samekh) in Aramaic. Our form (*śr'*) would correspond to Arabic *saru'a* " to be quick " and that is clearly useless to us. If our *šr'* were taken not as a cognate of this Ar. *šr'* but as an Ar. loanword in the local Aramaic, the reading *'tr* would become very doubtful.

[2] *AG* suggest the Latin *via*.

In l. 5 we took it for granted that the general clause *wrṯh l' yyty lk 'my bgw drth dk* included the denial of the right of way to the buyer. Here in l. 10 the same clause is followed by an explicit injunction to that effect: *wl' m'l [w]l['] mpq 'ly*.

Perhaps this—the general clause together with the special one—was the usual formula, and, having erroneously omitted the second part, Ḥdr may now have inserted the complete clause here. On the other hand, it is also possible that Ḥdr introduced the special clause as an additional safeguard.

The 2nd pers. of the pronoun at the end of the line must be a slip—it seems very unlikely that the 2nd pers. should have preserved the final vowel.[1] The 1st pers. is therefore probably the correct reading.

This linguistic argument is confirmed by the contents.

(1) The reading of *'nth* as the 2nd pers. would mean a reversal of the rôle between speaker and addressee in the deed. And that seems most unlikely.[2]

(2) The pronoun and the name Ḥdr conclude the whole twofold clause; this end must therefore correspond to the end of each of the halves, *'m*[*y*] and *'ly*, i.e., the pronoun must also be in the 1st pers. Even if it were taken to belong to the second half only, the same would apply. Apart from that, it follows immediately on *'ly*, but is at a great distance from *lk*.[3]

Does the fact that Eleazar has no right of way in the courtyard perhaps imply that his old house bordered on it—or was, at least, close to it and so situated that the way through the courtyard might be a short cut? This would fit in with our inference, in the discussion of l. 4, that his old house was adjacent to his new one. Accordingly, the old house would have been to the east or west of the courtyard.

Line 11

At the beginning of the line, the upper part of the traces might be the first stroke of the ligature *min*—and *min* is the word to introduce the phrase *ymh dnh wl'lm* which follows.

The difficult sequence of letters in the middle of the line can be defined as a word, because we have clear words before and after it: *'nh* and *'rb*, respectively. One uncertainty, however, remains: does the last of those letters belong to the word in the middle or to *'rb*?

If to the latter, that would give us either *y'rb* or *w'rb*. *Y'rb* is unlikely,

[1] It had already disappeared in the *EP*, six centuries earlier.

[2] In *EPK* No. 5 there appears to be such a reversal but there we have what are really two separate deeds: one is a manumission by a certain Meshullam, the other a paramone by a certain Tpmt.

In a few cases a grammatical change occurs: *'nh* is replaced in the course of the document by the name of the speaker (*EPC* Nos. 5 and 15, *EPK* Nos. 2 and 7. A change from the 1st to the 3rd pers. does not, of course, reverse the role of speaker and addressee.

[3] Cf. to *'nth* also *AG*.

because it is in the 3rd pers. and this does not fit in with the 1st and 2nd pers. throughout our deed. If *'rb* is taken as a verb, the same holds good for *w'rb* (unless *'rb* is interpreted as an imp.—which we do not get in deeds). Hence *'rb* is most probably a noun. If the preceding letter is *w* " and ", then the word in the middle would also be a noun. But neither *'hw* nor *'hy* can be read as nouns. Thus the waw/yodh must belong to the required word, and we have a choice between *'hww*, *'hwy*, *'hyw* and *'hyy*. The second is obviously correct: " I shall be "—*'hwy 'rb* " I shall guarantee ".

Line 12

The words *ḥdr/ . . dnh* after the tear must obviously have been preceded by " the wife of " (*M*). The form would be that with nûn, as suggested by *KBK* and two out of the three occurrences on ossuaries.[1]

The form of the renunciation corresponds to that in the Mishna (*R*). The expression for " law-suit, claim " (*mlyn*) corresponds to the second half of *dyn wdbrym* in the Mishna, while the *EP* have *dyn wdbb*.

Line 13

Something appears to have gone wrong in this line. If the clause was constructed like that of her husband in l. 11, the time should be mentioned at the end of the clause. We may therefore assume that the first *l'lm* is an error—a double error: wrong position in the sentence and the omission of *mn ymh dnh*—and when the writer reached the word *dk*, he realised it but was already so confused that he again omitted *mn ymd dnh*.

Since *bth* occurs so often in this deed the]*th* on the left of the big hole might be a remnant of the same word. For the preceding word I have no suggestion.

Could *nqnh*—which is presumably a pa"el: " to bring into possession, to sign over "—be connected with the next line, indicating that the transfer of the property was done by means of this deed? Or might it indicate a special form of transfer, as, e.g., the *qnyn swdr*?

Line 14

If the passage with *nqnh* is a clause concerning the method of transfer, the first half of l. 14—i.e., the end of the body of the document—might be a section of its own, dealing with the form of the document.

Our deed is of the " plain " or " open " type: the signatures of the witnesses are not on the outside of the closed sheet but " inside ", i.e., at the end of the text.[2] The words that follow *pṡyṭ* are therefore simply a duplication.

[1] *'ntt 'l'zr, śpyr' 'ntt śm'wn* (but *'lyśb' 'tt ṭrpwn*).

[2] Cf. *R*, *M* and *KBK 2*.

Shalom's signature follows immediately on the conclusion of the text. We may nonetheless assume that she was not the first to sign but that her husband had already done so at the beginning of the next line. Perhaps she did not like to leave a blank space at the end of the text. Or perhaps, in a feminine way, she put the signature where you would not quite expect her to put it.

There must have been a hê at the end of the line, as the lady would have used the fem. form of the verb.

Line 15

The restitution of nûn between lamedh and pe is obviously needed (*M*).

The expression *'l npšh* is known to us from the *BMD*.[1]

At first glance Judah's writing looks like that of the deed. But we cannot be sure as the hê differs considerably from the many specimens in the text. On the other hand, the writing of the document is pretty irregular—clearly not the hand of a professional scribe. So perhaps he is after all the writer of the deed. He would have copied the text from another deed.

After the name *'l'zr* a letter is missing. Among the possibilities for the next one there was resh (see Ch. I) so that what we have here might be the word *br*.

The next word starts with *mš* but the name Moses is out of the question: the letter after the shîn cannot be a hê.[2]

It stands to reason that the Eleazar of this signature is the Eleazar b. Eleazar of l. 2, the buyer of the house. Accordingly, if the second word is *br*, the next one starting with a mem, cannot be a name. We would then have to assume that Eleazar accidentally omitted his father's name.

The letters following his name—or the word *br*—presumably contain some kind of designation. Looking back to l. 2 we see the words Eleazar b. Eleazar followed by the sequence *šty'*. Here, in l. 15, we have almost the same, only that there is no waw/yodh between the teth and the aleph, and that the preceding mem might belong to this word.

Mšṭ' would then be a nominal derivation from the root *šṭ'*, and thus a parallel to the nominal form *šty'* of l. 2. The end of our word here is certain, because aleph has the final form.[3]

The next letters appear to constitute the word *memar*, i.e., a nominal form of the root *'mr*. Was this word in some way a parallel to Ḥdr's and

[1] *BMDB*, pp. 31 f.

[2] Apart from that, the name Moses was not current among the Jews of this period. The earliest occurrences I know of are in the 9th century: there is a gaon of that name at Sura, 825–836, and in the second half of the century there is Moses b. Aaron, the Masorete. (Could this be a case of Moslem influence? Musa was one of the Biblical names adopted by the Moslems from the very beginning. The earliest known bearer of that name seems to be Musa ibn Nusair, the conqueror of Spain [born 640].)

[3] Cf. *NS*.

Shalom's *'l npṡh*? The hê might be the suffix of the 3rd pers. masc. The meaning might be something like " matter; affair ". However, we would expect *'l* before it.

Line 16

As in the *BMD*, and on the ossuaries, Jehoseph, the long form of the name, is used.

In this line the only Hebrew word of the MS. occurs—as a technical term—*'d*. However, the next two witnesses use the Aramaic term.

Line 17

The first name consists of four or five letters, with a shîn in the middle. We have not a big choice: Jeshua and Manasseh occur in the *BKL*, *BMD* and *KBK*; the name Elisha was in use at the time: *'lyṡ' bn 'bwyh* and *'lyṡ' b'l knpym*. The traces before the shîn can hardly be squared with the letters of the name Manasseh or Elisha, even assuming a spelling *'lṡ'*. There is a better chance for Jeshua, spelt *yyṡ'*,[1] or perhaps Joshua, spelt *ywṡ'*. (In either case there was almost certainly no room for a waw before the ayin.)

The two surviving letters of the father's name leave no doubt that this name was Jehoseph—incidentally, one of the most frequent names in the four Murabba'ât documents so far published.[2]

Line 18

Two of the six persons named in this document have the same name as their father. There is one such case among the seven in the *BMD*. Three out of thirteen is certainly an unusually high percentage. If the custom of naming only a posthumous son after his father was already established at that time, this high percentage might mean that the three fathers in question had been killed in the war of 68–70. In the year of our deed, 134, their sons would be 64–66 years of age.

Reconstruction

To simplify matters we have, in general, omitted the dots above the letters used in the Temporary Transliteration (pp. 119f.).[3]

We have separated the words (cf. p. 119) but have not supplied any punctuation at this stage. That will be done at the next step in our interpretation—in the Tentative Translation.

[1] The *BKL* has *yṡ'*, the *BMD*: *yṡw'*.

[2] Here is a list of the names occurring in them, together with the number of occurrences: Eleazar (5), Judah (4), Jehoseph (3–6), Simeon (3), Ariston, Ḥdr, Jacob, Manasseh, Shalom (fem.). (Eliashib is probably not a personal name.)

[3] { } superfluous letter (or word) erroneously added by the writer. <> letter (or word) accidentally omitted by the writer.

1 בעסרה לאדר שנת תלת לחרת ישראל בכפר בביו

2 חדר בר יהודה מן כפר בביו אמר לאלעזר בר אלעזר שטיא

3 מן תמן אנה מן רעותי יומא דנה זבנת לך ימה דנה לבתה די לי

4 די פתיח [ל]צפון לגה דרתי די תפתחנה לגה בתך

5 ורשה לא ייתי לך {לך} עמי בנו דרתה דך זבנת לך בכסף

6 זחין די המון תמניה סלעין תרתן דמין גמרין לעלמ רשי

7 אלעזר בזבן בתה דך אבניא ושריתׁ[א ו]הגריא כול די בה

8 עפרה דקרקעא תחמא בתה ד[ך די אנת א]ל[ע]זר זבנה מדנחא

9 והו אתׁ[ר] שׁבֿע וצפנה דרתה מערבא [ו]דרמא בֿה ורשה לא

10 איתי לך עמי בנו דרתה די לי ולא מעל [ו]ל[א] מפק עלי אנ{ת}ה חדר

11 [מן] ימה דנה ולעלמ ואנה אהוי ערב [בז]בן בתה דך מן ימה דנה

12 ולעלמ ואנה שלמ ברת שמעון א[נתת] חדר דנה מלין לאיתי

13 לי {ול[ע]למ} בזבן בתה דך ‹מן ימה דנה› ולעלמ -ת-[-(-)ב]תה ודי נקנה ל-(-)(-)ל

14 כ[ת]בה דנה פשיט וחתמ בגוה שלמ [בר]ת שמעון על נפשה כתב[ה]

15 חדר בר יה[ו]דה על ‹נ›פשה אלעזר(-)- משטא מימרה

16 שמעון בר יהוסף עד

17 יש[ע] בר [יהו]סף שהד

18 [י]הודה בר יהודה שהד

Tentative Translation

1. On the tenth of Adar of the year three of the Freedom of Israel, in the Village of Bebhayu.

2. Ḥdr, son of Judah, of the Village of Bebhayu, says to Eleazar, son of Eleazar, *šty'*,

3. of the same place: Of my own free will {to-day} I sell you to-day the house I own

4. which opens on the north into my courtyard so that you will have entry through it into your house

5. but you shall have no right with me in this courtyard. I am selling for the amount of

6. eight zuz, i.e., two sela, full price, for ever. Entitled is

7. Eleazar through the purchase of this house to the stones and the beams (?) [and] the bricks (?) and all that is in it< from the height of the firmament to >the dust of the ground. The boundaries of the house which you, Eleazar, are buying, are: On the east side

9. it is the sloping ground (?), on the north side the court yard, on the west and south sides . . . . . And no right

10. have you (in common) with me in the courtyard which I own, nor right of way on me, Ḥdr,

11. [from] this day onwards forever. And I guarantee the purchase of this house from this day onwards

12. and forever. And I, Shalom, the daughter of Simeon, the wife of the said Ḥdr, have no claim

13. {forever} in connection with the purchase of this house <from to-day> and forever . . . . . [hous]e and which we sign over to (you) . . .

14. This document is open and signed within. Shalom, [daught]er of Simeon wrote on her own behalf.

15. Ḥdr. son of Judah, on his own behalf. Eleazar *mšṭ'*, in his affairs(?).

16. Simeon, son of Jehoseph, witness.

17. Jesh[ua], son of [Jeho]seph, witness.

18. [J]udah, son of Judah, witness.

Appendix

Spelling

The spelling is very fluid: a word is sometimes written in two quite different spellings in the same line, e.g., *ywm'* and *ymh* (l. 3)—a change between *scriptio plena* and *defectiva* for the first vowel, alternation between aleph and hê for the second vowel. The etymological principle is sometimes neglected: *lgh* occurs side by side with *bgw*.

Scriptio Defectiva

Scriptio defectiva may be employed for all long vowels: *bth*, *dk*, *ḥrt*, *zbn*, *ḥtm*, *ymh*, *ṯḥm'*, *trtn*. It is not used in final position.

Scriptio Plena

In medial position *scriptio plena* is of about the same frequency. However,

its use in final position and the spelling of the ending *yn* give the text, on the whole, an appearance of being in *scriptio plena*.

Aleph designates *â* or *ê* in final position: *'bny'*, *hgry'*, *ywm'*, *l'*, *mdnḫ'*, *m'rb'*, *qrq'*, *ṯḫm'*.

Hê designates *â*, *ê*, *ô* or *û* in final position: *'nh*, *bth*, *gh*, *dnh*, *drth*, *ymh*, *k[t]bh*, *nqnh*, *'srh*, *'prh*, *ṡpnh*, *rṡh*, *tmnyh*.

Waw designates *o*, *ô*, *û* and *u* (?) in either position: *bbyw*, *gw*, *hw*(?), *hmwn*, *zwzyn*, *kwl*, *r'wty*.

Yodh designates *î* in either position and *ê* in final: *'hwy*, *'yty*, *gmryn*, *dy*, *dmyn*, *drty*, *zwzyn*, *ly*, *mlyn*, *sl'yn*, *'my*, *pṡyṭ*, *ptyḫ*, *r'wty*, *rṡy*.

In reverse tabulation we have:

â is rendered—at the end only—by hê: *'nh*, *bth*, *dnh*, *drth*, *ymh*, *k[t]bh*, *'srh*, *'prh*, *ṣpnh*, *tmnyh*. But aleph is not much less frequent: *'bny'*, *hgry'*, *ywm'*, *l'*, *mdnḫ'*, *m'rb'*, *qrq'*.

ê is rendered by yodh: *'hwy*, *rṡy*; by aleph: *ṯḫm'*; by hê: *nqnh*.

î is rendered by yodh: *'yty*,[1] *gmryn*, *dy*, *dmyn*, *zwzyn*, *ly*, *mlyn*, *sl'yn*, *'my*, *pṡyṭ*, *ptyḫ*, *r'wty*.

O and *ô* are represented by waw: *gw*, *hmwn*, *kwl*, in one word by hê: *gh*.

û and *u* (?) are rendered by waw: *bbyw*, *hw* (?), *zwzyn*, *r'wty*, in one word by hê: *rṡh*.

[1] *'yty* (l. 5) is not a matter of spelling. The first yodh reflects the glide which replaced the glottal stop between the â and *î* of *lâ 'îtai*. (Cf. Kutscher apud *AG*, and the *BKL*, where the glottal stop also disappeared but without necessitating a glide: *'alai 'et* > *'alaiet*.)

ISRAELITE AND PRE-ISRAELITE PLACE-NAMES IN PALESTINE: A HISTORICAL AND GEOGRAPHICAL SKETCH

By B. S. J. Isserlin

It is a well known fact that the place-name map of a country is apt to be a reflection of its history: the occupation of the national territory by various ethnic groups in the course of time is often mirrored in the various kinds of designations of localities. Thus the place-name map of modern England (to take but one example) contains names of Celtic, Latin, Saxon, and Scandinavian origin. Each of these groups of names is connected with a special phase in history, and interesting conclusions can be drawn from an analysis of the distribution of the various types of place-names.

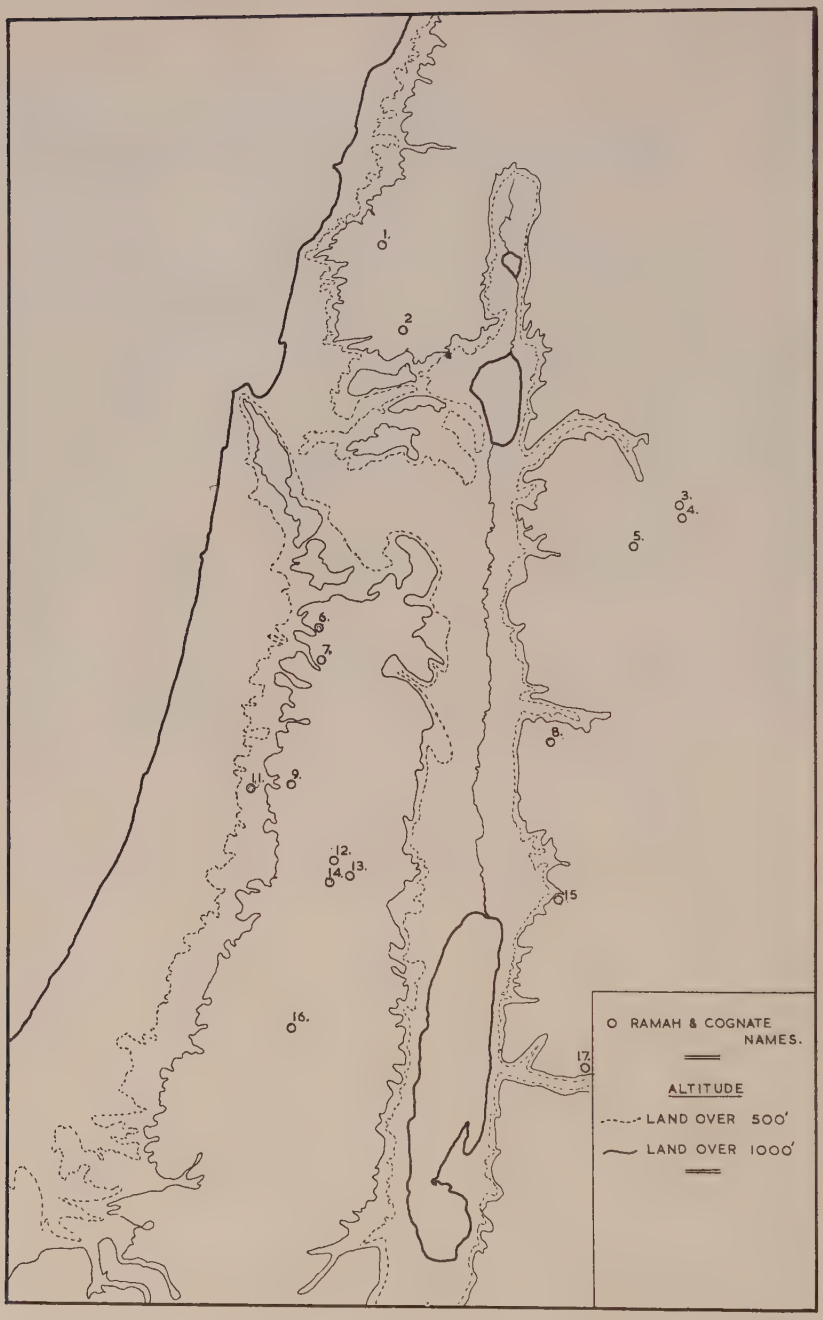
To a person concerned with ancient Palestine, and more particularly with the crucial event of Hebrew immigration, the question will present itself whether it may not be possible to shed some light on the arrival and settlement of the Israelites by a study of Palestinian place-names. That question has up to now been answered mainly in the negative. W. Borée, whose book *Die alten Ortsnamen Palästinas* is still the only complete and systematic study of all the data available at the time of publication (1930), seemed inclined to think there was, not a break, but rather an essential continuity between Israelite and pre-Israelite topographic nomenclature.

Since the time when Borée wrote new data of various kinds have accumulated, and it may be permissible now to re-open the question, attacking it from a somewhat different angle and in a somewhat different way.

We might begin, like Borée, by collecting the various name materials, both Israelite and pre-Israelite (i.e., the place-names contained in the Execration Texts, the Amarna Letters, the Egyptian royal lists, the Old Testament, the Assyrian records, etc.). These data could then be ranged into various *classes* (thus, e.g., Beth-Shemesh and Beth-Shan would be members of the class Beth-x; while Migdal-Gad and Migdal-Eder would form part of the class Migdal-x). We could then proceed to arrange the various classes of names in a time chart, according to their appearance in the records.

If this is done it will become apparent that:—

(1) Some classes of place-names were in active use during both the Israelite and pre-Israelite periods: this seems true in the cases of Beth-x, 'Ain-x, Abel-x, *inter alia*—though x in Israelite names refers perhaps less frequently than before to divine beings.


1.
2.
3.
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11.
12.
13.
14.
15.
16.
17.
O RAMAH & COGNATE NAMES.
ALTITUDE
LAND OVER 500'
LAND OVER 1000'

Fig. 1

(2) Some names seem to flourish more during the Israelite period than earlier. This appears to be the case with names formed from the root *gbʿ* (Gibeah, etc.).

(3) Certain classes of names are not attested in the records prior to the Israelite period. These names include Baʿal-x, Mizpeh-x and Mizpah/eh, ʿAtaroth-x and cognate forms, and Ramah, Ramath/oth-x.

Of these classes, Baʿal-x can be ruled out for our present enquiry: it is not a typically Israelite name, but occurs both in Israelite and Phoenician territory. The other items under headings (2) and (3) should however be directly relevant for our purpose.

If we proceed to examine what archaeological data are available for the presumed sites of places bearing names in classes (2) and (3) then it will appear that in a fair number of cases the archaeological findings reported refer to the Late Bronze and Iron Ages only.[1]

If, furthermore, we proceed to plot on a map of Palestine these presumed sites of places bearing names in classes (2) and (3), then another fact will become apparent. We are dealing essentially with *upland names*, which tend to avoid the coastal plain belt. Most names belonging to classes found in the pre-Israelite period, on the other hand, are met both in the uplands and in the lowlands. (There are indeed some exceptions to this rule: thus the name class Hazor, attested as early as the Execration texts,[2] seems largely confined to the highland zone.) Cf. maps 1–5.

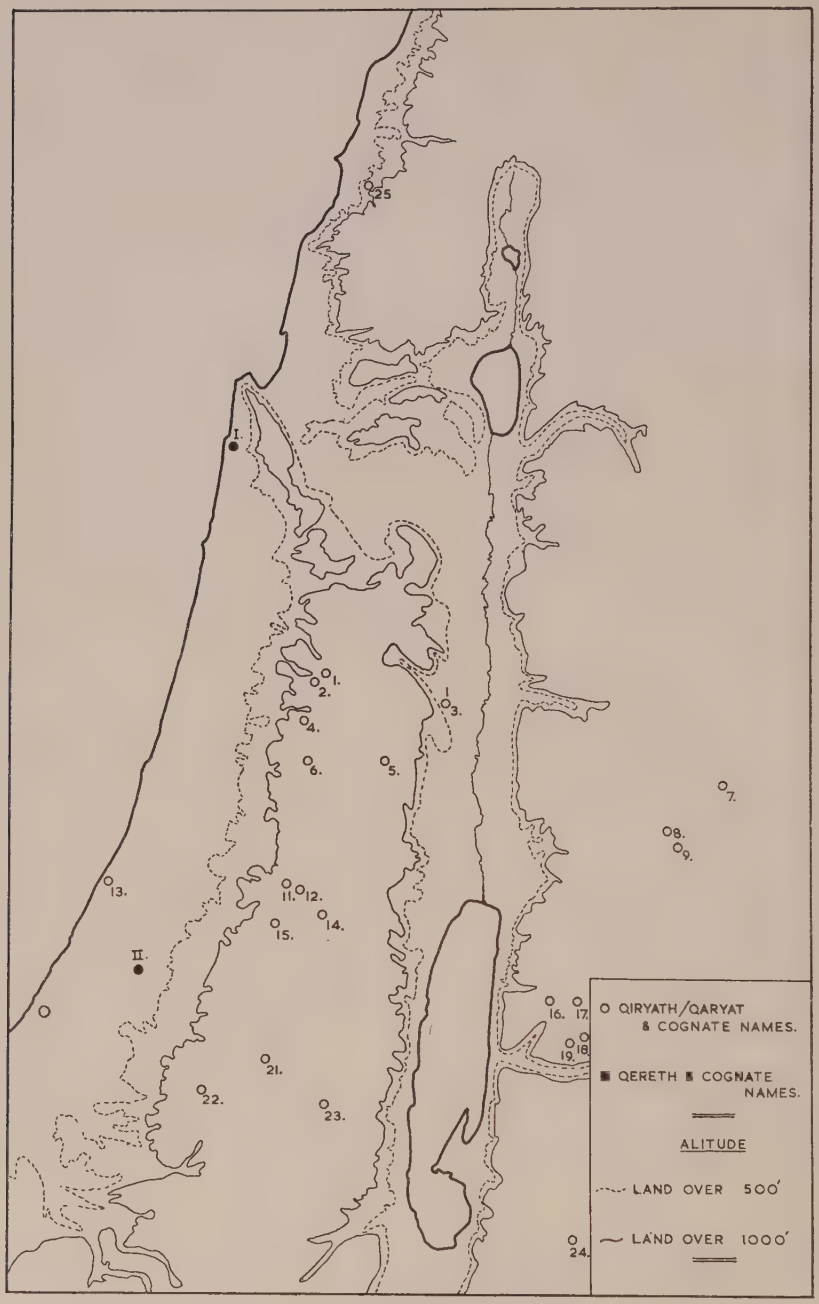
It would thus appear:—

(*a*) that names in classes (2) and (3) are attested in historical records only from the time of Israelite settlement and occupation in Palestine;

(*b*) that in a fair number of cases the localities concerned cannot be

[1] In the case of places whose names are derived from the root *gbʿ*, Gibeah of Saul/T. el-Ful dates from the transition from the Bronze to the Iron Age (W. F. Albright, *B.A.S.O.R.* 4, 1924, cf. *B.A.S.O.R* 52, 1933, pp. 6–12) though Gibeon/el-Jib was founded early in the Bronze Age (*ILN*, October 27th, 1956, p. 697 (ca. 3,000 B.C.). In the case of the class Mizpah/eh, we might refer among probable sites to T. en-Nasbeh, re-occupied in the Iron age after a desertion so prolonged that any memory of an older name seems unlikely (Wampler and McCown, T. en-Nasbeh I, pp. 53–4), and to the Iron Age foundation Kh. el-Kibdeh, identified by N. Glueck with Mizpeh of Gilead (*A.A.S.O.R.* XXV–XXVIII, pp. 61, 65). In the case of Ramah and associated forms, Ramoth-Gilead can be traced back to the Late Bronze Age if this town should be located at el-Husn (N. Glueck, *A.A.S.O.R.* XXV–XXVIII, p. 96); if er-Remtha, or T. er-Ramith represents the ancient town, it would go back to the Iron Age (Glueck, *ibid.*, pp. 97–8). Ramah/er-Ram likewise seems to go back to the Iron Age (M. Noth, *Palästina-Jahrbuch*, 1935, p. 16). As for the class ʿAtaroth, etc., the locality in Ephraim (Josh. xvi, 7), if localized at T. Sheikh edh-Dhiab (Abel, *Géographie de la Palestine*, II, p. 255) would go back to the Late Bronze Age (A. Alt, *P.J.B.*, 1927, p. 31); if it is to be located at T. el-Mazar it would be an Iron Age foundation (N. Glueck, *A.A.S.O.R.*, XXV–XXVIII, p. 418 ff.). (Perhaps Ataroth-Adar, if at Rafat, is of that time (*P.J.B.*, 1926, p. 10).)

[2] F. Posener, *Princes et Pays d'Asie et de Nubie*, p. 73, E 15.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QIRYATH/QARYAT & COGNATE NAMES.
QERETH & COGNATE NAMES.
ALITUDE
LAND OVER 500'
LAND OVER 1000'

Fig. 2

traced back archaeologically before the period when the Israelites settled down in the country;

(*c*) that they are largely restricted to the very upland belt which was at all times the home ground of the Israelite confederation and kingdoms.

All this would seem to raise a strong presumption that these name classes can to a fair degree be ascribed to Israelite settlers.

It must be said at once that typically Israelite place-names are in any case not confined to the above groups. Thus the Geders and Hazers (as against Hazors) are also largely of this time, so far as their attestation in records goes, perhaps also to some extent archaeologically, though geographically the division is harder to establish.[1] Again, building on older foundations, the name class Qiryath-x (like its modern opposite number Qaryat-x) is essentially restricted to the highlands, while forms doubtfully linked with the Phoenician form *qereth* occur in the coastal belt.[2] Typical of the period of the monarchy are lastly many upland names describing concrete features, like Bachurim, Gallim, Betonim, Ma'on.[3] In this connection it is of interest to note the apparent survival of pre-Israelite divine names in what were apparently Israelite foundations (Anathoth, perhaps also Bachurim).[4] There is on the other hand only one doubtful example of a settlement named after the national god of the Israelites, YHWH, and this occurs in a text referring to Transjordan at the end of the Bronze Age.[5] It looks as if YHWH, a jealous god, did not lightly permit his name to dwell in any place; whereas the older deities were an unconscionable time in dying, as the prophets knew to their cost.

Up to now we have restricted our field of enquiry to Palestine only; but we must test our findings against the wider background of the place-name map of the Ancient Near East.[6]

[1] As for Hazer, we might refer to Hazar Shu'al, if this is to be located at Kh. el-Meshash, an Iron Age foundation (A. Saarisalo, *J.P.O.S.*, IV, 1924, p. 153). The root *gdr* implied in the names Gedora in Perea, would there occur in a settlement dating to the Hellenistic period at least (R. de Vaux, *R.B.*, XLVII, 1938, p. 402); Gedor/Jedur seems to be an Iron Age foundation (W. F. Albright, *B.A.S.O.R.* 18, 1925, p. 9, cf. M. Noth, *P.J.B.*, 1934, p. 42).

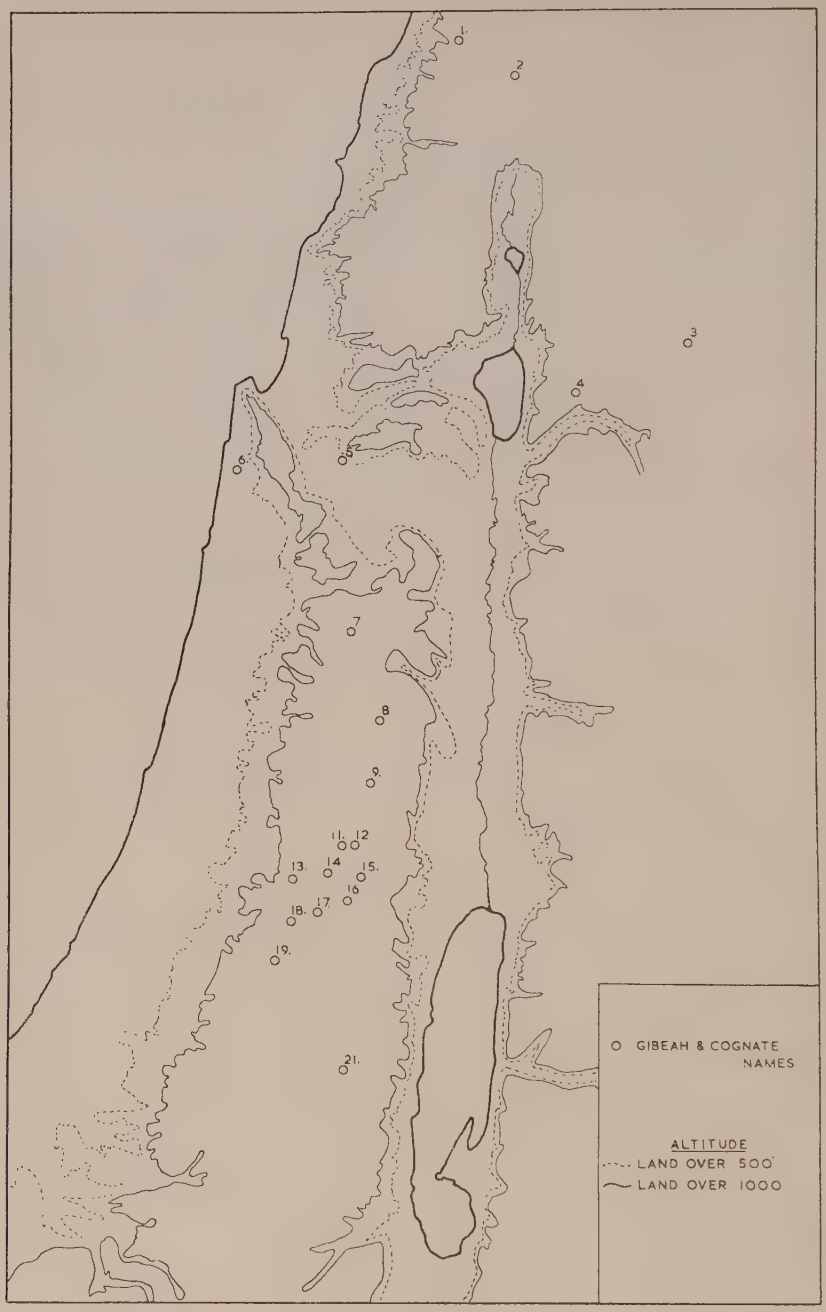
[2] We might refer to Atlith, ancient Certha (C. N. Johns, *Q.D.A.P.*, III, 1934, p. 151) and just possibly to Qeratiya in the southern coastal plain.

[3] For the dating of these, cf., in the case of Ma'on, A. Alt, *P.J.B.*, 1934, p. 35; for Betonim, cf. R. De Vaux, *R.B.*, XLVII, 1938, p. 404; for Gallim, cf. A. Alt, *P.J.B.*, 1926, p. 22; and for Bachurim, see *ibid.*, p. 32. All these sites are attributed to the Iron Age.

[4] For Anathoth, cf. W. F. Albright, *B.A.S.O.R.* 61, 1936, p. 23; A. Bergman, *B.A.S.O.R.* 62, 1936, pp. 22–5; 63, 1936, p. 22. For Bachurim, *vide supra.*

[5] B. Grdseloff, *Revue de l'Histoire Juive en Égypte*, I, 1927, pp. 79 ff., esp. p. 81.

[6] For a comparative treatment of the name groups in the ancient Near East the reader is referred to my essay in the *Proceedings of the Leeds Philosophical Society* (Literary and Historical Section), vol. VIII, 1956, Part II, pp. 83–110, where an attempt is made to sketch the historical development of West Semitic topographic nomenclature.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O GIBEAH & COGNATE NAMES
ALTITUDE
LAND OVER 500'
LAND OVER 1000

Fig. 3

If we go over the list of name types contained in classes (2) and (3), looking for non-Palestinian parallels, results seem to be as follows:—

(*a*) No foreign parallels seem discernable for 'Ataroth and cognate forms.

(*b*) In the case of Mizpah/eh and cognate forms, it is likewise difficult to be sure of analogous forms outside Palestine.[1]

(*c*) Gibeah and cognate forms occur sporadically in the high Lebanon, perhaps elsewhere in Syria.[2]

(*d*) Localities from the stem *ram* are not so rare: they occur mainly on high ground in Syria.[3] They seem thus to be concentrated in territory once under Aramean sway, and the name Ramah has indeed been claimed as Aramaic on philological grounds.[4] Such connections would not agree badly with the often mentioned " Aramean " strain in Israel's ancestry; as for the other name classes, there seem to be no sufficient indications obliging us to refer them to a general West Semitic rather than Israelite context.

Yet if the attribution of the name groups mentioned above to Israelite settlers has any truth, then some further conclusions about the character of Israelite immigration seem to impose themselves.

Firstly, our findings seem to point to some original unity overriding the cleavage between Israel and Judah, north and south. The idea is not infrequently mooted that Judah and Ephraim were by origin two essentially different groups: Ephraim was truly Israelite, but Judah largely of Canaanite stock, and their eventual union, partial at best, was the result of the slow drawing together of disparate groups under the pressure of historical events.[5] Yet our findings seem not to support this view. On the maps, it will be noted, the indicator groups straddle the north and south fairly impartially. Both seem thus to have had a fair share of the immigrants who imposed these name types. The biblical accounts which speak of a close kinship between the immigrating tribes everywhere would thus seem to have something in their favour.

The detailed development of Israelite settlement, the spread of tribal or political units, it seems difficult to trace at present on the strength of place-name material. Yet a few modest beginnings can perhaps be made. Thus the cluster of names from the root *gb'* in the highlands just north of Jerusalem

[1] W. F. Albright has recently drawn attention to the occurrence of the root *spy* in Aramaic names, but gives no further details: cf. *B.A.S.O.R.* 143, 1956, p. 33.

[2] See the remarks *ad loc.* in the above-mentioned paper, *Proc. Leeds Phil. Soc.*, VIII, 1956, p. 107, n.9.

[3] See the maps published in R. Dussaud's *Topographie Historique de la Syrie*, from which several examples can be culled.

[4] H. Bauer and P. Leander, *Historische Grammatik der Hebräischen Sprache*, p. 24.

[5] As for instance in Th. Meek, *Hebrew Origins* (2nd ed.), pp. 43 ff; T. H. Robinson, *History of Israel*, I., pp. 119 ff., 135; cf. also H. H. Rowley, *From Joseph to Joshua*, pp. 140 ff., for other somewhat analogous attitudes, and some criticisms.

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● MISPAH & COGNATE NAMES.
ALTITUDE
LAND OVER 500'
LAND OVER 1,000'

Fig. 4

(map 3) seems inexplicable in terms of topography alone: we may here be dealing with the results of regional colonization from one centre. Similarly the occurrence of the two names Shilo and Ta'anath Shilo at a moderate distance from each other may hint at the original existence of some regional unit in that part of the Samaritan uplands. Careful study of local problems of this sort may one day help to make the process of Israelite origins more easily understandable.

Secondly, it should by now be possible to attack the question of Israelite settlement, at least in broad outline, in terms of historical geography, linking the locations of the various place-name types with their occurrence in geological, soil, and botanical belts.[1] By and large, settlements in the uplands are in the zone characterized by terra rossa and grey soils, and original Mediterranean scrub forest of various sub-varieties. There seems in fact to be a possibility that some of the Geders and Gederahs, 'Ataroths, Ramahs and Qiryaths are on cleared forest ground. (As against this the forests originally covering the Kurkar ridges along the northern part of the coastal plain seem to have been left pretty well alone.) This would conform with what is said in Joshua xvii, 15 ff., or II Chron. xxvii, 4. The economic pressure forcing a surplus population into the foundation of new settlements on forest soil was effective elsewhere in driving them to habitation in other less promising types of environment. There are a number of new Israelite foundations in the steppe or semi-steppe belts, and the Hazers in particular seem to reach into the poor country of southern Judea where conditions called for small scattered communities; Ma'on may be similarly understood. Many of the upland names seem to refer to various kinds of herdsmanship, others to fruit growing or the tending of vineyards. Of more advanced occupations there is little indication. This agrees with the geographical background, and also with what is implied in the biblical accounts dealing with the period of the judges and early monarchy.

Notes on the maps.—These are in every case plotted so as to show (*a*) what seem to be the most likely locations of ancient sites bearing names of the classes in question; (*b*) they also show modern name-forms which *might* be descendants of names of the class under discussion. Where no exact location seems indicated, sites are not shown; so far as can be seen, this omission does not essentially vitiate results.

Key to map 1 (*Ramah and cognate names*)

(1) Ramieh, (2) Rameh, (3) er-Remtha, (4) T. er-Ramith, (5) Husn/

[1] For convenient reference we would point to M. Zohary's Geobotanical Soil Map of Western Palestine (*Palestine Journal of Botany*, Jerusalem Series, IV., 1, 1947, pp. 24–35) and the same author's Vegetation Map of Western Palestine (*Journal of Ecology*, XXXIV, 1947, pp. 1–19).

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ALTITUDE
LAND OVER 500'
LAND OVER 1000'
GEDER & COGNATE NAMES.
ATAROTH " " " "
HAZOR " " " "

Fig. 5

Ajlun, (6) er-Rameh, (7) Ramin, (8) Kh. Jela'ad/Ramath Hammizpeh?, (9) Beit Rima/Arimathea?, (11) Rentis/Arimathea?, (12) Ramallah, (13) er-Ram, (14) Nebi Samwil, (15) T. Iqtanu/Beth Haram, (16) er-Rameh and Ramath el-Amdeh near Hebron, (17) Rameh.

Key to map 2 (Qiryath/Qaryat and cognate names)

(1) Qaryet Jit, (2) Qaryet Hajja, (3) Qarawa el-Mas'udy, cf. ancient Corea, (4) Qarawa Ibn Hasan, (5) Qariyut, (6) Qarawa Ibn Zeid, (7) Qureyat Hadid, (8) Qureyat Nafi, (9) Qureyat Salim, (11) Qiryath Ye'arim, (12) Qaryat el-Enab, (13) Cariath Maus, (14) Qaryat Saideh, (15) Kh. Erma (Qiryath Ye'arim?), (16) Qureiyat/Qiryathaim, (17) Qaryet Malih, (18) Qureiyat Alyan, (19) Qaryat Falha, (21) Hebron/Qiryath Arba, (22) Debir/Qiryath Sefer, (23) Qeryoth Hezron.

(I) Qeratiya, (II) Kh. Dustrey or Atlith/Certha.

Key to map 3 (Gibeah and cognate names)

(1) Jeba'a, (2) Jebain, (3) T. el-Jabieh, (4) Jibin, (5) Jebatha, (6) Jeba/Geba', (7) Jeba/Geba', (8) Awertha/Gibe'ath Pinehas? (9) et-Tell nr. Sinjil/Gibe'ath Pinehas? (11), Ramallah/Gibe'on?, (12) el-Bireh/Gibe'on?, (13) Jibi'a, (14) el-Jib/Gibe'on? (15) Jeba'/Geba', (16) T. el-Ful/Gibe'ath Shaul, (17) Kh. Jubei'ah, (18) Kh. Jeba', (19) Jeba/Gabatha, (21) Gibe'ath Hahakilah.

Key to map 4 (Migdal/Mejdel and cognate names, and Mizpah/eh)

(1) Mejdel esh-Shems, (2) Mujeidil/Migdal El? (3) Kh. el-Mejdel/Migdal El?, (4) Mejdel Islim, (5) el-Mejdel, (6) el-Mujeidil, (7) el-Mejdaliyeh, (8) el-Mejdel/Magdala, (9) Mejd el-Kerum, (11) el-Mejdel, (12) el-Mujeidil, (13) Kh. Malha/Migdal Malha, (14) el-Mejdel, (15) el-Mejdel, (16) el-Mejdel, (17) Mejdel Beni Fadl, (18) Mejdel Yaba, (19) Kh. Mejdala, (21) Kh. el-Beydat/Magdalsenna, (22) Migdal Eder, (23) el-Mejdel, (24) Kh. el-Mejdeleh, (25) Kh. Megdel Ba'a, (26) Kh. el-Mejadil and T. el-M., (27) Kh. Mujeidilat, (29) Mejdelein, (30) el-Mejadil.

(I) T. er-Remtha, (II) Suf/Mizpeh Gilead?, (III) Kh. Jelaad/Ramath Hamizpeh?, (IV) Kh. Qasr es-Sur/Ramath Hamizpeh?, (V) T. en-Nasbeh/Mizpah?, (VI) Nebi Samwil/Mizpah?, (VII) Rujm el-Musheirifeh/Mizpeh?, (VIII) Masfa/Mizpeh.

Key to map 5 (Geder and cognate names, Hazor, etc., and Ataroth, etc.)

(1) Jidru/Gedora, (2) Gadara, (3) es-Salt/Gedorah?, (4) Kh. el-Judeira/Gedor, (5) Kh. Jedireh/Gederah, (6) Qatra/Gederoth?, (7) Kh. Judraya, (8) Jedireh/Gederah, (9) Kh. Jedur/Gedor.

(I) Azur, (II) Kh. Hazireh, (III) T. el-Qedah/Hazor, (IV) Kh. Hazzur, (V) el-Azzeir, (VI) Asiret el-Hatab, (VII) Hazar, (VIII) Asiret el-Qibliyeh,

(IX) el-Azeir, (X) T. Asur/Ba'al Hazor, (XI) Kh. Hazzur, (XII) el-Qureitun/Hazor (Hadatta?) Kh. Hezeir.[1]

(1) Attarah, (2) T. el-Mazar/Ataroth Adar?, (3) Sheikh edh-Dhiab/Ataroth Adar?, (4) Kh. Auja el-Foqa/Ataroth Addar? (5) Attarah, (6) Attarus.

Note. Since this article went to press, the American excavations at el-Jib have rendered the identity of that place with ancient Gibeon practically certain (*R.B.* LXIV, 1957, p. 228). Nos. 11 and 12 on Map 3 can therefore be deleted

[1] Modern names tentatively given in this class include forms with initial ', ' or *h*, all of which might be descendants of ancient ה. Names with medial *ī* instead of *ū* could be diminutives, but also derivatives from the kindred name-class Hazer.

CORRESPONDENCE

Sir,—I should like to correct my recent article in *P.E.Q.*, 1957, pp. 68 f., concerning the inscribed sealing from Jericho (*JPO* 5.1, # 1291). The photograph has been examined by Dr. William F. Albright of The Johns Hopkins University and I am indebted to him for the following changes.

First of all, my original reading of the first line (ṢGND) is quite impossible. The letter originally read as an (early) *tsade*, is actually a combination of an initial *lamedh* and a second letter whose upper portion is apparently lost. Dr. Albright has suggested a (post-exilic) *he* in the latter place. The letter originally seen as a *gimel* is thus the other half of the incomplete *he*. The third letter, originally read as *nun*, is still bothersome because of what appears, in an enlargement of the original photograph, as an additional stroke on the upper bar of the letter. It could still be read as *nun*, but a *gimel* appears preferable. The final letter is likewise not absolutely clear. It was originally seen as *daleth*, because of its difference from the indisputable *resh* of the second line. However, allowing for a certain variation in letter formation, especially in seal carving, *resh* would be equally possible as a reading.

With the above corrections made, the first line of the sealing would thus be LḤGR, denoting ownership of the original seal by a female bearing a quite plausible Biblical name.

The dating must be revised in the light of Dr. Albright's criticism, as well. In communications on the impression, he has pointed out that the lateness of the *waw* and *resh* of the second line, combined with the late *he* of the first line, and the spelling *-yw*, lead him to suggest a date for the seal in the third century B.C.

If the above dating holds, this would make the Jericho sealing one of the latest examples of this seal type, and an important contribution to the corpus of post-exilic, non-pictorial seals.

A re-publication of the original article, incorporating the above changes in detail, including comparative material, will be presented in the October, 1957, issue of *The Bulletin of The American Schools of Oriental Research.*

Yours truly,

P. C. Hammond

16*th* July, 1957

REVIEWS AND NOTICES

Atlas of the Bible. By L. H. Grollenberg, O.P. Translated and edited by Joyce M. H. Reid, B.A., and H. H. Rowley, D.D., F.B.A. Thomas Nelson. 1956. Pl. 166. 14 x 10½ ins. 70*s*. net.

The last few years have yielded quite a rich harvest of helps for students of the Bible and biblical archaeology. We have had the excellent *Westminster Atlas of the Bible*, Professor Pritchard's two invaluable volumes of ancient Near Eastern texts and pictures illustrating the Old Testament, Professor Ernest Wright's *Biblical Archaeology*, *The Times's* publication *The Bible Today*, and now, to crown all comes a book of which it is difficult not to speak in superlatives. The verse comes to mind, "Many daughters have done virtuously, but thou excellest them all." Although the book is called an atlas it is much more than a collection of maps with an index. In the words of the eminent biblical archaeologist Père de Vaux, who has written an introduction to the book, "the Atlas makes use of three media in close conjunction: maps, illustrations, and text. Not only do the maps indicate relief, political frontiers, and the sites of towns mentioned in the Bible; by a system of colours, symbols, and over-printed legends, they succeed in expressing the events which took place in this geographical setting; they spring to life and each becomes a page of history." The author, who is a member of the famous Dominican School of Biblical and Archaeological Studies at Jerusalem, and has himself worked as a field archaeologist under Père de Vaux, has had the happy idea of making the prophetic conception of Israel as a person the basis of his treatment of Israel's history. He has told the story of Israel's birth and infancy, her youth, her independence, backsliding and punishment, and finally her death and resurrection. Each stage is illustrated by maps of remarkable accuracy and wealth of descriptive detail, and the maps are supplemented by photographs of sites and excavations, scenery, monuments, inscriptions, many of them taken by Father Grollenberg himself and of quite unusual excellence. There are also a number of remarkable air-photographs which give a vivid impression of such scenes as the macabre Dead Sea landscape, the wild highlands of Judaea, or the foothills of the Lebanon. The air-photograph of Jerusalem on p. 114 deserves special mention, fronting on the opposite page a fine map of the city in the time of Christ. The text, too, is a masterpiece of accurate and vivid compression: nothing essential has been omitted. The much overworked adjective "indispensable" can be applied to this wonderful book with full realization of its meaning and appropriateness. The translation is excellent, and Professor Rowley has added another item to the vast debt which all students and lovers of the Bible already owe to him, for making this wonderful

production available for English readers. Messrs. Nelson, too, are to be congratulated on the technical and artistic skill with which the book has been produced.

S. H. Hooke.

Second Thoughts on the Dead Sea Scrolls. By F. F. Bruce. London, The Paternoster Press, 1956. Pp. 144. 10*s.* 6*d.* net.

Since the first news of the discovery of the Qumran documents, commonly called the Dead Sea Scrolls, ten years ago, a literature has sprung up round them as rapidly as Jonah's gourd, and some of it perhaps as ephemeral. Professor Dupont-Sommer's rather hasty interpretation of some obscure passages in the Scrolls led some writers to the equally hasty conclusion that the new documents were destined to bring about a revolution in Biblical studies in a sense unfavourable to the claims of Christianity. To any lay reader who may have been disturbed by these wild statements, the present unassuming book by Professor Bruce should bring reassurance. Behind the modest title and appearance of the book there lies a sound and ample scholarship which enables the authority to speak with authority, and to set forth with clarity the dividing lines between what is certain and what is uncertain in our present state of knowledge concerning the contents of these remarkable documents. Professor Bruce inclines to the opinion that the references to the Kittim and their worship of their standards date the Habakkuk Commentary in the Roman period, whereas other scholars, including Professor Rowley, would place it in the Seleucid period, and here there is much to be said on both sides. But discoveries still continue, and the interpretation of what has already been discovered is still in an early stage, so that more light may be hoped for on various disputed issues. Meanwhile no better "Guide for the Perplexed" can be recommended than this excellent, sane, and wholly scholarly discussion of the contents of the Dead Sea Scrolls. It may be added that Professor Bruce's Tyndale lecture on *The Teacher of Righteousness in the Qumran Texts* has been published by the Tyndale Press (price 2*s.*), and is a most valuable discussion of one of the most important elements in the Scrolls. It can be heartily recommended as an important contribution to the literature which has grown up round the Scrolls.

S. H. Hooke.

Mélanges Louis Massignon: Tome I. Damascus, 1956. Pp. xiv, 430.

Here is a choice collection of 18 essays by French-speaking Arabists and Islamologists. They have been gathered in loving and appreciative honour of a great scholar and influential Christian, who occupies a front-rank position as linguist, historian, interpreter. More important, M. le Professeur has been a welcome guest these fifty years in the homes of friends scattered through

two score countries. Some of these are numbered among the contributors.

The book is published under the patronage of the Institute of Islamic Studies of the University of Paris and the French Institute of Damascus. It is no mere accident that it saw the light in 1956, proving that spiritual and cultural relations are not severed when economic and diplomatic are broken off. But not only Damascus comes into the picture. Massignon owes much to Baghdad. He has left a legacy of learning in Cairo, Jerusalem, Algiers and other cities, sacred and less so, as M. Henri Massé states in the Foreword. If the " mystic " in Massignon is revealed in his study of Hallāj, he is " littérateur " *par excellence* in Quranic exegesis and Islamic folklore, and always up-to-date in the contemporary political and religious scene. Like Raymon Lull before him he has hovered alike over Paris and North Africa. Like Lull too his interest in books and ability for lectures has never outweighed his concern for people—students of course in particular! When he visited the Newman School of Missions in Jerusalem in the 'thirties and entered the " Prayer Room " at the close, he remarked, " It is good to study beneath the shadow of the Cross ".

Those responsible for this volume (which is to have a successor) have done well in giving first place to a " Massignon Bibliography ".

This is really a literary " biography ". Arranged in chronological occurrence, these publications stretch from 1906 to 1955, 207 books and articles in all. Even then but the half is told, for the range in realm of catholicity extends to the compilation of reports and the editing of Arabic texts.

But the heart of the book (almost unreviewable!) lies, as Massignon would have it, in the studies which are offered as " hommage ". The subjects dealt with are as wide as Islam—and Massignon. Names and themes are familiar to the student of Islam. We have not reached the end of research into al-Ghazzali, still less into the corpus of *Hadith*. Here too is something to enrich philology and mysticism. Nor is poetry forgotten. There is interest for the student of the Life of the Prophet in the essay concerning the Farewell Pilgrimage. The controversy of Ibn Hazam is balanced by the rising interest in Greek classics in the modern Arab East. Persia, Turkey, Morocco, and the nomadism of Southern Algeria have something to contribute. The study on " The Onager, the Zebra and the Marabou(t) " will be rewarding to others besides the zoologist. Essays that are the work of specialists demand specialists' reviews. The studies are carefully documented and worthy of the teacher the writers " delight to honour ".

M. Massé refers to Orientalists who meant much in the life and work of Louis Massignon, household names for all students of Islam, Lévi, Goldziher, Snouck-Hurgronje. The trio has become by common consent a quartet. Though French and Arabic are the languages of the lands Massignon knows best, it might be considered whether the successor volume could contain studies in German or English; for scholarship and sainthood are no national

monopoly, evoking as they have in Massignon's career the learning that is born of friendship.

E. F. F. Bishop.

A Genesis Apocryphon. A Scroll from the Wilderness of Judaea. Description and Contents of the Scroll; Facsimiles, Transcription and Translation of Columns II, XIX–XXII. By Nahman Avigad and Yigael Yadin. Jerusalem, 1956. The Magnes Press of the Hebrew University and Heikhal ha-Sefer. Pp. 48 (English text), 40 (Hebrew text), 14 (facsimiles and transcription). $13\frac{1}{2}$ x $9\frac{1}{2}$ ins.

With the appearance of *A Genesis Apocryphon* we are within sight of the publication of the last of the documents found in Cave 1 at Qumran ten years ago. This is the fourth scroll originally acquired by the Syrian Monastery of St. Mark in Jerusalem—the scroll which for a time was tentatively identified with the lost *Apocalypse of Lamech*, the title of which has been preserved in two manuscripts (one in the Coislin collection and one in the Vatican Library). The tentative identification was based on a detached fragment of the scroll which contained the words (in Aramaic): "Then I, Lamech, hastened to go in unto Bit'enosh." Since Bêtênosh appears as the name of Lamech's wife in *Jubilees* iv, 28, the provisional suggestion that the work was the *Apocalypse of Lamech* was not unreasonable.

Now, however, it is plain that the scroll contains part of an expanded and imaginative paraphrase of Genesis, in which the successive stages of the Old Testament narrative are related by some of the chief actors. In the section to which the detached fragment belonged (col. 2), Lamech describes the birth of his son Noah. He tells how the child at his birth was so exceeding fair that he suspected that one of the heavenly Watchers must be his father, until he was solemnly assured by Bit'enosh that it was not so. The story resembles the account of Noah's birth in 1 Enoch cvi, 1; the newly published *apocryphon* evidently came from the same milieu as the Enoch literature and *Jubilees*.

A good part of the story of Abraham is told by Abraham himself. Apart from the detailed enumeration of Sarah's charms (col. 20), which "hit the headlines" when news of the contents of the scroll was first released, one of the most interesting features of the story of Abraham is the expansion of Gen. xiii, 17, where the patriarch is instructed by God to walk through the land which he was to receive as an inheritance "in the length of it and in the breadth of it." The *apocryphon* tells (col. 21) how Abraham, in literal obedience to this command, started from the "River Gihon" (probably an eastern arm of the Nile Delta) and went north along the Syrian and Phoenician coast as far as "the Mount of the Ox" (Aram. *ṭûr tôrâ*, i.e., the Taurus range); then he turned east to the upper Euphrates, went down the Euphrates to the Persian Gulf, and continued to travel round the Arabian coast until he came back to his starting-point. In the paraphrase of

Genesis xiv, Arioch appears as king of Cappadocia (*kptwk*) and the territory of Tidal, king of nations, is located " between the Rivers " (col. 21, ll. 23 f.). The incident is dated ten years after Abraham's departure from Haran (col. 22, ll. 27 f.).

The process of deciphering the remaining columns will be a long one, because of their poor state of preservation. This is due not only to extensive rotting of the leather but also to some corrosive ingredient in the ink which was used on it. We are very grateful to the editors for giving us this interim report; we should be grateful, too, to Mr. Biberkraut who undertook the delicate task of separating the layers which had become practically glued together, and of opening the scroll so that it could be read. We wish them all success in the work that must yet be completed before an edition of all the surviving text is published.

The document is valuable not only as a contribution to the history of Jewish literature and biblical exegesis; it will also be studied with great interest for the light that it may throw on the evolution of the Aramaic language.

The introduction to the scroll and translation of the text are given both in English and in Hebrew.

F. F. B.

Jewish Apocalyptic and the Dead Sea Scrolls. By H. H. Rowley. University of London: The Athlone Press, 1957. Pp. 36. 4*s*.

Here we have the text of the Ethel M. Wood Lecture delivered before the University of London on March 12, 1957. Earlier lectures in this series have been given by Professors Margaret Deansly, C. H. Dodd, C. S. Lewis and T. W. Manson; and Professor Rowley has made a distinguished contribution to a distinguished series. Here, by relating the major Qumran documents to the wider field of Jewish apocalyptic, he defends the thesis which he has already propounded from other viewpoints (*cf.* " The Kittim and the Dead Sea Scrolls ", *P.E.Q.*, July–December, 1956), that the decisive personalities and events in the history of the Qumran sect belong to the second century B.C.

F. F. B.

Mission de Ras Shamra. Tome VIII. *Ugaritica III.* Par Claude F.-A. Schaeffer, avec des contributions de Chr. Desroches-Noblecourt, H. S. Güterbock, P. Krieger, E. Laroche, O. Masson, J. Vandier. Paris: Librairie Orientaliste Paul Geuthner, 12 Rue Vavin (VIe), 1956. 11 × 8½ ins. Pp. xi, 304, 10 plates, 239 figures. 4,000 fr.

The latest volume (the eighth) of *Mission de Ras Shamra* to reach us is the third in the series *Ugaritica*. In it M. Schaeffer and his colleagues publish material of the utmost value for the study of Ugarit's relations with the Hittite world, and also with Egypt and Cyprus. The material includes

Hittite seals, cylinders and other documents, a sword with the cartouche of Merneptah engraved on it, and Cypro-Minoan tablets.

The picture of the relations between Ugarit and the Hittite world is one of constant vigilance on the part of Ugarit in face of Hittite alarms and excursions on her border. Ugarit was, however, able to use her wealth to build strong fortifications and maintain a well-equipped army; the mountain rampart to the north (where Baal, her tutelary deity, was believed to dwell) was a further protection; and, if the worst came to the worst, there was always the last resort of an escape by sea to the friendly shores of Cyprus or Egypt. The Hittites, on their part, must have realized that a raid on Ugarit, however momentarily successful, would bring no long-term advantages to them; it might, indeed, lead to an appreciable diminution of the tribute from those regions which swelled the coffers of Hattusas. Thus the kings of Ugarit succeeded in maintaining their sovereignty in spite of the menacing proximity of so powerful a neighbour.

British readers will take special pleasure in the fact that M. Schaeffer has prefaced this volume with a graceful dedication to Professor and Mrs. Mallowan.

Twenty-Five Years of Mesopotamian Discovery. By M. E. L. Mallowan. British School of Archaeology in Iraq, 5 New Square, Lincoln's Inn, London, W.C.2. Pp. 80, 3 plates, 18 figures and maps. 5*s*.

The British School of Archaeology in Iraq was founded at a meeting held in London on January 14th, 1932. To mark the Silver Jubilee of the School, Professor Mallowan has produced this informative and well-illustrated booklet. He discusses the sites at which archaeological research has been carried out by expeditions sponsored primarily by the School and published in its journal *Iraq*. (Incidentally, he may well take satisfaction in claiming " as an achievement of the School that none of its work has gone unpublished.") The sites in question are Arpachiyah, Chagar Bazar, Brak, the Balikh Valley, Nimrud and Balawat. The account of Balawat (an ancient mound 15 miles E.S.E. of Mosul, which was investigated in April, 1956) contains a vindication of Hormuzd Rassam's claim that the magnificent bronze gates of Shalmaneser III recovered by him in 1877 came from that site—a claim on which doubt was thrown by a number of scholars who could not believe that such splendid trophies could have been found in so small a place. The investigations of 1956 brought to light another set of bronze gates, belonging to Shalmaneser's father, Assur-nasir-pal II.

We must congratulate the School on its fine record during its first quarter-century, and wish it increasing success and prosperity as it presses on with its good work. And we hope that Professor Mallowan's account will stimulate practical interest in the work of the School among the ever widening public which is paying such keen attention to archaeological discovery.

The Committee have to acknowledge with thanks the following:—

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American Journal of Archaeology. Vol. 61, Nos. 1–3, 1957.
American Journal of Philology. Vol. LXXVIII Nos. 1 and 2, 1957.
Annual of the Dept. of Antiquities of Jordan. Vol. III, 1956.
Annual Report of Smithsonian Institute, 1955.
Antiquaries Journal. Vol. XXXVII, Nos. 1–2, 1957.
Bible Lands. Vol. XIII, Nos. 13–14, 1957.
Biblica. Vol. 38, Nos. 1–2, 1957.
Biblical Archaeologist. Vol. XX, Nos. 1–2, 1957.
Biblical Research Monthly. Vol. XXII, Nos. 1–8, 1957.
Bulletin of the American Schools of Oriental Research. No. 144, 1956, 145 and 146, 1957.
Bulletin of the Israel Exploration Society. Vol. XX, Nos. 3–4, 1956, Vol. XXI, Nos. 1–2, 1957.
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Ex Oriente Lux. 1955–56. Jaarbericht No. 14.
Expository Times. Vol. LXVIII, Nos. 5–10, 1957.
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Internationale Zeitschriftenschau für Bibelwissenschaft und Grenzgebiete. Band IV, Heft 1–2, 1955–56.
Israel Exploration Journal. Reprint from Vol. 6, No. 4, "New Record of Nebuchadrezzar's Palestinian Campaigns." By A. Malamat. Vol. 7, Nos. 1–2, 1957.
Jewish Quarterly Review. Vol. XLVII, Nos. 3–4, 1957. Vol. XLVIII, No. 1, 1957.
Journal of the American Oriental Society. Vol. 76, No. 4, 1956. Vol. 77, Nos. 1–2, 1957.
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Journal of Egyptian Archaeology. Vol. 42, 1956.
Journal of the Royal Anthropological Institute of Great Britain and Ireland. Vol. 85, Parts I–II, 1955. Vol. 86, Parts I–II, 1956. Vol. 87, Part I, 1957.
Journal of Semitic Studies. Vol. II, Nos. 1–3, 1957.
Kush (Journal of the Sudan Antiquities Service). Vol. II, 1954.
Miscellanea Comillas. Vol. XXIV, 1955. Vol. XXVI, 1956.
Nea Zion. July–Sept., 1956. Jan.–Mar., 1957.
Nouvelle Revue Theologique. Vol. 78, No. 10, 1956. Vol. 79, Nos. 1–7, 1957.
Revue Biblique. No. 4, 1956. Nos. 1–2, 1957.
Scripture. Vol. IX, Nos. 6–7, 1957.
Syria. Vol. XXXIII Hommage—M. Chas. Virolleaud, 1956. Vol. XXXIV Fascicules 1–2, 1957.
Zeitschrift des Deutschen Palästina-Vereins. Band 72, No. 2, 1956.

OBITUARY

GRACE MARY CROWFOOT

Grace Mary Crowfoot, who died at her home, the Old House, Geldeston, Norfolk, on March 20th, was a remarkable, many-sided person. She was born at Nettleham Hall, in Lincolnshire, on November 3rd, 1878, the eldest daughter of Sinclair Frankland Hood. In 1909 she married John Winter Crowfoot, and most of the next thirty years of her life were spent in the Middle East, where her husband worked as an educational administrator and archaeologist, and where her four daughters were born. In Egypt and the Sudan, and later in Palestine, she became immersed in the life of the people, particularly the women, whose crafts she studied and whose problems she understood. In the Sudan she is above all remembered for the part she played in starting the Government maternity service, and in introducing Miss Wolff to organize the training of Sudanese midwives.

A countrywoman born and bred, Mrs. Crowfoot was keenly interested in Natural History, and some of her earliest published work was in this field: *Some Desert Flowers*, composed in Egypt; *Flora of the Sudan* (which forms the illustrated section of the official handbook); and *From Cedar to Hyssop*, a study of the folk-lore of plants in Palestine. Her archaeological career began before her marriage, when as a young woman she excavated a neolithic cave on the Italian Riviera. She naturally took an active part in her husband's major excavations at Jerash in Transjordan, and at Sebastiya (Samaria) in Palestine. Here her special contribution was work on the pottery and glass. During this Middle Eastern period she was increasingly drawn to the subject on which her reputation as a scholar chiefly rests—ancient textiles: an interest which grew out of observing and reproducing primitive weaving methods in the Sudan. (On one occasion, in order to learn the technique of the double weave, she succeeded in inducing a Bedu woman of the Hadendowa to travel five days by camel to Omdurman to teach her.) From the 1920's on, Mrs. Crowfoot contributed many articles to learned journals on topics bearing on this theme—camel-girths, tablet weaving, Egyptian and Sudanese looms, Tutankhamen's tunic, the belt of Rameses. She was responsible for the section on the textiles from Sutton Hoo for the British Museum Catalogue; the study of the linen wrappers of the scrolls found in the Dead Sea caves; and the chapter on the tablet-woven braids in the magnificent volume on the relics of St. Cuthbert, recently produced by the Dean and Chapter of Durham Cathedral.

During the last twenty years of her life Mrs. Crowfoot was increasingly sought after as consultant, collaborator, and lecturer. She gave the Munro

lectures on primitive weaving at the University of Edinburgh in 1951, and wrote the chapter on " Textiles, basketry and mats " in the first volume of Dr. Charles Singer's *History of Technology*. She was successful in combining these major pieces of work with an active life in Geldeston itself. The Parochial Church Council, the Labour Party, the Village Produce Association, a great circle of neighbours, friends and family, all looked to her for help and advice. Geldeston flowed through her house and garden. She would turn, with the same single-mindedness and concentration, from delicate work on the linen fragments from Qumran to comforting a grandchild, or judging a school-child's wild flower collection at the V.P.A.'s annual fête.

T. L. H.

I first met Mrs. Crowfoot at the beginning of the first season at Samaria. When I arrived, the camp was set up and all the organisation in being. It was typical of Mrs. Crowfoot that it was not till long afterwards I realised what this all implied; domestic arrangements all running so smoothly that one took them for granted, everything in order for the recording and examination of finds, a warm welcome for all newcomers, and all with a minimum of fuss.

She was the ideal coadjutor for a Director of Excavations. Not only did she undertake most of the routine organisation at the dig headquarters, both archaeological and domestic, but she was a first class archaeologist herself. Whereas her husband was concerned with the broad interpretation of history and structures, she was prepared to devote endless time and trouble to the minutiae.

As a colleague, she was exacting. She had an absolutely amazing memory for details. If one attempted to argue a point without all the facts at one's finger-tips, one was immediately flattened. But one could not feel any resentment, for she was almost always right, and when one could produce an effective answer, she always accepted it. Above all, her enthusiasm was infectious. Years of labour on meticulously exacting and minute studies never dimmed her interest, which she could pass on to all working with her.

I have two last memories. One is of the last lecture I heard her give. It was to the P.E.F. on the textiles from Qumran. The subject was technical in the extreme, and one on which her audience had no expert knowledge. But her enthusiasm was such that she held us for every minute of the lecture. The second memory is of the last time I saw her. Already a very sick woman, she still was able to discuss the final details of the third Samaria volume with all her old flair and energy, and with still an absolutely clear recollection of all the material, even when she had not handled it for years. She was a very great woman, but the note on which I would prefer to end is that of the affection she inspired.

K. M. K.

JACOB ORI

Mr. Jacob Ori, mentioned by Dr. Eva Danelius in her current papers on "The Boundary of Ephraim and Manasseh in the Western Plain" as the discoverer of the mound of Mekmish, died in March of this year, about sixty years of age. Mr. Ori grew up in the land, and served the Department of Antiquities (first under the mandatory regime and then under the Government of Israel), being inspector for the southern region most of that time. While he had no academic training in archaeology, he knew every square yard of the coastland. Dr. Danelius writes: "When he sat beside me and told me of his observations, I learned more than from a hundred articles. He encouraged me in my present study, discussing the various points, and especially Mekmish, which was his favourite. At that time I still objected to his identifying the mound with the city of Reshef, and I looked forward to his surprise when he read my article! He realized that he himself would never live to solve the problems presented by the various mounds, and was on that account the more eager to help and encourage a younger student. He had the great advantage of intimate knowledge of the Arabs, who told him all they knew and looked on him as a real friend".

ANNUAL GENERAL MEETING, 1957

The 91st Annual General Meeting of the Palestine Exploration Fund was held at 2 Hinde Street, Manchester Square, W.1, on July 11, 1957, at 5.30 p.m. (Mr. G. M. FitzGerald in the Chair).

The Minutes of the 90th Annual General Meeting were read, approved and signed.

The Chairman then said:

" It now falls to me briefly to report the events of the last twelve months. I regret to say that amongst the members of the Fund who have passed away I have to record such well-known names as Professor Garstang, Colonel Newcombe, Lady Petrie and Mrs. Crowfoot.

" I need hardly remind you that Professor Garstang, after making his name as an excavator in Egypt and Asia Minor, became the first head of the British School in Jerusalem and of the Department of Antiquities in Palestine, and subsequently initiated the excavations at Jericho which are being so successfully carried on by Miss Kenyon. Professor Garstang made many ties of friendship in Palestine, where his memory is still held in much honour. His death occurred last September on board ship in the harbour of Beirut. He had been invited as guest of honour on a cruise which was to give him the longed-for opportunity of revisiting the site of Mersin, in Cilicia, where, while Mrs. Garstang was still alive, he had carried out a brilliantly successful dig before the last war. Only two days before his death, in spite of great weakness, he realized his wish; he was able to come ashore, to revisit the site and to acknowledge the cheers of the members of the cruise who gathered round him. It was a scene which I shall never forget.

" By the death of Colonel Newcombe the Executive Committee lost an old and valued member. As an officer in the Royal Engineers he carried out a survey of Southern Palestine in 1914; afterwards he served with Colonel Lawrence in the Arabian Revolt. We owe him a debt of gratitude for helping to bring about the agreement under which the Royal Central Asian Society now shares these rooms, an arrangement which is working very satisfactorily.

" Lady Petrie maintained to the end of a long life a deep interest in the Archaeology of the Holy Land. She will be remembered as the zealous and energetic helpmate of Sir Flinders Petrie during many years of work in Egypt and between the wars in Palestine.

" I am sure you will sympathize with Mr. Crowfoot and his family in their recent bereavement. Thirty years ago I had the privilege of living at the British School in Jerusalem with Mr. and Mrs. Crowfoot and ever afterwards greatly valued their friendship. Mrs. Crowfoot came to take a great interest in the archaeology of Palestine, and there, as well as earlier in the Sudan and

at her home in East Anglia, she had wholehearted concern for the welfare of the people amongst whom she was living. She was particularly interested in their handicrafts and as regards textiles was a recognized authority—some of you may have noticed her contribution to the recent publication of those from St. Cuthbert's shrine, at Durham. During the last year of her life the forthcoming publication of the finds from Samaria absorbed her attention, and her friends have thought it appropriate that tributes to her memory should take the form of subscriptions to the publication of Volume III of *Samaria-Sebaste*, the cost of which weighs heavily on the resources of the Fund.

" Our income does not at present suffice for contributing to field-work in Palestine. Nevertheless I think such activities as we can afford are by no means negligible. These headquarters, which house our library and collection of slides, are available to, and made use of by, a considerable number of seekers after knowledge. The *Quarterly* continues its half-yearly appearance.

" During the last twelve months we have had to thank a number of scholars for kindly consenting to give an interesting series of lectures. We were greatly obliged to Mr. Barnett for arranging for us to visit the exhibition, at the British Museum, of antiquities from Iraq, and to Professor Mallowan for explaining the exhibits. The lecturers we have welcomed here have been: Dr. Avigad (on Herod's Palace of Masada), Professor Evenari (on Agriculture in the Nabatean Period), Dr. Yadin (on his excavations at Hazor), Miss Tufnell (on Dress and Ornament in Biblical Times), Mr. Barnett (on Ancient Shipping and the Phoenicians), Miss Seton Williams (on Excavations at Tell Rifa'at) and Miss Kenyon (on Excavation Life at Jericho).

" In this connexion I should like to congratulate Miss Kenyon very warmly on her enterprise in going out this spring to resume, if only for a short time, the Jericho excavations, and to superintend the establishment in a home of its own of the British School of Archaeology in Jerusalem. I may mention that I received a very appreciative letter on this subject from the doyen of Palestinian archaeology, Père Vincent. I am sure all members of the Fund will wish the School every success, and for my part I hope the relations of Fund and School will be as close as it is possible for them to be."

The Honorary Treasurer's Report, given by Mr. Thompson, together with the audited accounts, were approved by the meeting.

The Executive Committee as at present constituted, with Colonel Walmsley White filling an existing vacancy, was elected for the coming year.

Messrs. Turquand Youngs were again unanimously appointed the Fund's Auditors.

The Chairman then asked for the meeting's approval for the sale to the British Museum of the original drawings by the late Lord Kitchener (made when he was a Lieutenant) of the maps on which the Survey of Palestine maps were made. It was pointed out that the British Museum map section was in a better position than the Fund to preserve these drawings. The meeting approved unanimously.

THE PALESTINE EXPLORATION FUND

BALANCE SHEET, 31st DECEMBER, 1956

1955							1955						
£	*Capital:*	£	s.	d.	£	s.	d.	£	*Fixed Assets:*	£	s.	d.	£	s.	d.
	Authorised—200 Shares of £1 each ..	200	0	0					Freehold Property—Donated:						
	Issued—9 Shares of £1 each, not called..	—			—				No. 2, Hinde Street (estimated value £5,000)				—		
	Surplus of Assets over Liabilities:								Alterations and Improvements:						
	(Excluding donated property)							1,189	No. 2, Hinde Street at net book value 1st July, 1948				1,188	17	5
2,651	Amount as per last Balance Sheet ..	2,845	7	6					Museum, Library and Storeroom, Fixtures, Fittings, Furniture, etc., at						
	Add: Excess of Income over Expendi-							382	net book value 1st July, 1948 ..				382	2	7
194	ture for year..	290	17	8					Typewriter at cost	39	10	0			
2,845					3,136	5	2		*Less:* Depreciation to date	28	0	0			
								15					11	10	0
								—	Otzar Hityashvuth Hayehudim B.M. Israeli £250 Ordinary Stock (Registered) Donated				—		
									Jewish Colonial Trust—Donated:						
									50 Ordinary Shares of 2s. 6d. each,						
								—	fully paid				—		
								1,586					1,582	10	0
	Current Liabilities:								*Current Assets:*						
396	Sundry Creditors for Current Expenses	377	12	3					Stocks of Publications, Maps, Books, Models, Lantern Slides, Library and						
121	Subscriptions in advance	73	7	0				—	Antiques				—		
63	Rent received in advance	62	10	0				10	Subscriptions in Arrear	11	3	0			
357	Donations toward cost of Publication	882	8	10					Investments—Post Office Issue—at cost—						
					1,395	18	1		£374 12s. 0d. 3½% Conversion Loan 1961 or after .. £ 400						
									£50 3% Savings Bonds 1955-65 50						
									£264 1s. 8d. 2½% Savings Bonds 1964–67 250						
									£300 3½% Defence Bonds 300						
									£100 2½% Defence Bonds 100						
								1,100	(Market Value £925) ——	1,100	0	0			
								48	Sundry Debtors	77	17	8			
									Cash at Bank, on Current Account						
								1,012	and in hand	383	11	5			
								—	Cash at Bank on Deposit Account ..	1,350	0	0			
									Cash on Deposit—Post Office Savings						
								26	Bank	27	1	2			
													2,949	13	3
£3,782					£4,532	3	3	£3,782					£4,532	3	3

AUDITORS' REPORT TO THE MEMBERS OF THE PALESTINE EXPLORATION FUND.

We have examined the above Balance Sheet and annexed Income and Expenditure Account which are in agreement with the Fund's books of account and we have obtained all the information and explanations which, to the best of our knowledge and belief, were necessary for the purposes of our audit. Proper books of account have been kept by the Fund, so far as appears from our examination thereof.

In our opinion and to the best of our information and according to the explanations given to us, the said Accounts give the information required by the Companies Act, 1948, in the manner required and present a true and fair view of the state of affairs as at 31st December, 1956, and of the excess of Income over Expenditure for the year.

TURQUAND, YOUNGS & CO.,
Chartered Accountants.

19, Coleman Street, London, E.C.2.

THE PALESTINE EXPLORATION FUND

INCOME AND EXPENDITURE ACCOUNT FOR THE YEAR ENDED 31st DECEMBER, 1956

1955		£ s. d.	£ s. d.
£			
874	Printing—Quarterly	704 13 0	
—	Miscellaneous	23 5 6	
874		727 18 6	
60	*Less:* Contribution by British School of Archaeology in Jerusalem	60 0 0	
814			667 18 6
5	Library		1 10 0
605	Salaries, Wages and Pensions		605 4 0
31	Repairs		39 13 0
22	Insurance		25 19 6
56	Rates		112 1 6
33	Lighting and Heating		42 0 0
53	Postage and Telephone		57 8 5
80	Stationery and Sundries		56 11 5
11	Audit Fee		10 10 0
4	Depreciation on Typewriter		3 10 0
194	Balance being excess of Income over Expenditure		290 17 8
	Note: £100 has been included in respect of remuneration to a member of the Executive Committee.		
£1,908			£1,913 4 0

1955		£ s. d.	£ s. d.
£			
615	Subscriptions and Donations, including local Societies		786 19 2
72	Refund of Income Tax on Covenanted Subscriptions		59 10 5
459	Sales of Publications		289 4 0
725	Rents		725 0 0
	Dividends Received—Gross:		
	On £374 12s. 0d. 3½ per cent. Conversion Loan	13 2 2	
	On £50 3 per cent. Savings Bonds 1955–65	1 10 0	
	On £264 1s. 8d. 2½ per cent. Savings Bonds 1964–67	6 12 0	
	On £300 3½ per cent. Defence Bonds	10 10 0	
	On £100 2½ per cent. Defence Bonds	2 10 0	
33			34 4 2
3	Premium on Conversion of Defence Bonds		—
—	Interest received on Bank Deposit Account		17 13 3
1	Interest on Post Office Savings Bank Deposit Account		13 0
357	Donations towards cost of publishing *Samaria*, Volume III	525 0 0	
(357)	*Less:* Liability for same	525 0 0	—
£1,908			£1,913 4 0

In connection with the above Accounts the Executive Committee would like to point out that during 1957 the Fund will have to meet the publication costs of *Samaria-Sebaste* III. Despite generous donations this will put a heavy strain on the finances of the Fund, and it is for this reason that it has been necessary over the past two years to conserve the Fund's available resources.

(Signed) GERALD M. FITZGERALD, Chairman,
OLIVER H. THOMPSON, Hon. Treasurer.

11th July, 1957.

HARRISON & SONS, LIMITED,
By Appointment to Her Majesty The Queen
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